

DETERMINANTS OF EMOTIONALITY IN THE RAT

I. SITUATIONAL FACTORS

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An experiment was designed to establish the optimum conditions for using Hall's open-field test of emotionality in the white rat. A large ($N=192$) factorial design permitted simultaneous evaluation of the following components of the test which were thought to be productive of emotional responses: size of test arena, intensity of illumination and intensity of sound. The effects of pre-trial shock, food deprivation and sex difference were also investigated. Other variables were strictly controlled.

The results, principally analysed in terms of defecation and ambulation scores, show that females defecate significantly less than males and ambulate more, that noise is a potent factor in evoking emotional elimination and light less so. Arena size does not affect defecation, but rats run farther in a larger arena. Pre-trial shock reduces defecation, whereas food deprivation has no significant effect. Interactions are few, and mostly concern sex differences in response.

It is concluded that the test can yield sensitive indicators of an emotional response, and that this response is susceptible, within limits, to experimental control. The implications for the use of the test are discussed.

I. INTRODUCTION

Loss of control over elimination may be induced by fear. This has been shown (Stouffer, Guttman, Suchman, Lazarsfeld, Star, Shirley & Clausen, 1950, p. 142) to be the extreme fear symptom on the continuum of physiological symptoms reported by soldiers as occurring in battle conditions. Similarly, the rat will defecate or urinate or do both when placed in a strange, fear-provoking situation. As Munn (1950, p. 98) notes in his review of the work on emotional behaviour in the rat, observation of this tendency has long been used as a measure of emotionality (Yerkes, 1913; Stone, 1932). Systematic work began with the validation studies of Hall (1934, 1936*a*, *b*), who used a circumscribed arena or open field in which to observe it. His demonstration of the validity of defecation as an emotional index showed that it decreases with successive exposures to the same situation and has been repeatedly confirmed (e.g. Biel & O'Kelly, 1940; Farris & Yeakel, 1945). Factorial validity has also been demonstrated (Billingslea, 1942). A negative correlation between elimination scores and ambulatory activity in the open field is a feature of the results derived from this test (Hall, 1936*b*).

But the technique has been criticized because it is space-consuming (Lebo, 1953), because of the failure to obtain interpretable amounts of defecation (Griffiths & Stringer, 1952), and because such scores, when obtained, do not always correlate with other single purported measures of emotionality or timidity (O'Kelly, 1940; Bindra & Thompson, 1953; Hunt & Otis, 1953). The last authors suggest that the test should be more stressful. It is clear, therefore, that the fear-provoking components of the test should be identified and standardized, preferably while retaining the simplicity of Hall's original procedure. Accordingly, the present experiment has been designed to allow us to examine, independently, each of several components of the open-field test thought to be stressful, and to evaluate the effects of specific changes in their intensity.

Furthermore, in much animal experimentation, what might be called 'background

conditions, e.g. the strain of the subjects, their age, previous experience and husbandry, are not specified (Christie, 1951). Recent evidence shows that such conditions may well be determinants of behaviour in this test (Watson, 1954; Weininger, 1954). Thus, while the purpose of this study is to attempt the definition of the optimum conditions for using the test, the effects of background conditions cannot be ignored. It is proposed, therefore, to maintain such conditions constant for all animals, and to concentrate attention on the variables inherent in the testing situation itself, except for those which are imposed (sex and litter membership), or which it seems profitable to vary (hunger drive).

Illumination of the arena is the only test condition which has been systematically varied, Evans & Hunt (1942) finding that defecation scores increased with brightness. Noise has been employed as an emotional stimulus (Patrick, 1931; Bindra & Thompson, 1953); and its use is advantageous since the same noise at a lower intensity can serve as a sound screen, thus avoiding the need for sound-proofing the subjects against distracting external noises. The size of the test arena used has varied greatly, but, on *a priori* grounds, one might expect a larger arena to be more stressful for the somewhat agoraphobic rat. Methods of making the open-field test even more stressful for the rat (Anderson, 1939, 1940) are difficult to adapt to a large arena. The procedure adopted here is that of the 'pre-trial shock', a technique which may be expected to heighten anxiety. In addition to these four variables within the testing situation, it is proposed to investigate the effect of hunger drive, because Hall (1936*a*) found hungry rats to be less emotional. His claim that this effect could not be due to a smaller available bulk of faecal material (since urination scores were also affected) is invalidated by the evidence that food-deprived rats also drink less (e.g. Verplanck & Hayes, 1953). The evidence relating to sex differences in open-field test scores is both scanty and contradictory (Hall, 1934; Parker, 1939; Farris & Yeakel, 1945; Thompson & Bindra, 1952).

Certain predictions regarding the effect on open-field test scores of the experimental variables can be made, bearing in mind that increased elimination and decreased activity may be regarded as characteristic of the more emotional rat (Hall, 1936*b*). They may be summarized as follows:

larger arena	}	will result in greater elimination and less activity,
brighter light		
louder noise		
pre-trial shock		
food deprivation		will result in less elimination and more activity.

It will be appreciated that these expectations are tentative and have heuristic value only. In view of conflicting evidence regarding sex difference in scores, no predictions are made for the effects of this variable.

II. EXPERIMENT

(i) *Experimental design*

The selection of an analysis of variance design is determined by several considerations, prominent among which are the number of independent variables to be manipulated and the number of subjects available. In the present experiment, it is desired to evaluate the effect of five variables on the subjects' behaviour in the open-field test. To these five must be added two more, sex and litter membership, which are imposed. About 200 animals were available for the experiment. Since such a large number cannot be given the test on any one occasion, the variables of the subjects' age and the time of day of testing are inevitably introduced.

Five variables, each at two levels, plus sex, requires sixty-four animals, in order to assign one to each unique combination of treatments. The primary choice lay between using the remaining subjects for increasing the number of levels of treatments like intensity of light, noise, etc., and foregoing the advantages of replicating the design, or assigning more than one subject to combinations of treatment at two levels only. The latter alternative was chosen. Replication permits evaluation of any effects of age obtainable by varying systematically the mean age of the subjects assigned to each replication, within, that is, the small age range of 100–110 days here employed. Any effects of the time of day of testing—morning sessions as opposed to afternoon—can also be determined.

In order to investigate the effect of inter-litter differences, a design involving blocks was required. The one selected is the cubic lattice of Yates (1939). It is so called because each of the three replications requires k^3 subjects, where k equals the number of subjects in each block. In this experiment, blocks represent litters and $k=4$. In only seven cases (out of forty-eight) was one non-litter-mate required to complete a block. On the other hand, this equating of litters with blocks was necessarily imperfect in that we had only twenty-four litters to be distributed over the forty-eight blocks, and so some litters were distributed over two or even three blocks. The sixty-four combinations of treatments were randomly assigned to block positions which are balanced between each replication. The subjects were allocated to replications by age, to blocks by virtue of litter composition, and to cages (and hence to cage mates) at random within sex. Where this resulted in fewer than three rats in a cage, one or two non-experimental animals of the same age and like sex were used to make up the standard cage complement of three. The order of testing was determined by block membership, and the first eight blocks in each replication (thirty-two animals) were given the open-field test in the morning, the next eight in the afternoon.

(ii) Apparatus

Two sizes of open-field arenas were used in order to provide the two levels of treatment required for this variable. The larger arena was made of curved metal units which enclosed, when assembled, an area 6 ft. in diameter with a wall $12\frac{1}{2}$ in. high, the interior surface of which was painted flat white. It was bottomless and stood directly on a granolithic floor on the outermost of six concentric circles divided into segments by radial lines. These floor markings were needed to provide compartments of roughly equal size so that the ambulation could be easily scored as an animal moved around in the arenas.

The smaller arena was designed to go, when in use, inside the large arena and to span the three innermost circles. It had a diameter of $32\frac{3}{4}$ in. and was made of plywood for ease of handling, but was otherwise identical with the large arena. Around the sides and over the top of the large arena, a light metal structure supported a screen of muslin which served both as a one-way screen and to diffuse the illumination from above. The structure also supported the batteries of lamps and loudspeakers for providing the light and noise stimulation.

The intention was to have this stimulation distributed as uniformly as possible over the arenas. The light was provided by six photographic lamps wired in parallel and arranged at a height of 44 in. in a pattern that produced the most uniform illumination at floor level in either arena. Measurements with a Ferranti light meter (type LMB) showed a range of 170–195 foot-candles in the large arena, and 145–185 f.c. in the small arena. This constituted the high level of illumination. Switching a large resistor in series with the lamps reduced the intensity of illumination to 30–37 f.c. in the large arena and to 22–32 f.c. in the small. This constituted the low level of illumination. While these determinations were being made and during subsequent testing, all other illumination was excluded.

Continuous 'white noise', that is, noise of all frequencies at once, was fed in, via an amplifier, to six loudspeakers, $6\frac{1}{2}$ in. diameter mounted 51 in. above the floor. The source for the white noise was the output of a small commercial neon lamp (Pascal & Swenson, 1952). Appropriate resistors in the circuit (a potential divider) gave two levels of noise. The intensity of these two sound fields was measured by exploring the floor level of the arenas with a calibrated microphone (Acos, type MIC 2), the output of which was amplified and displayed on an oscilloscope (Cossor, model 1049). The wave form was observed to be characteristic of that for white noise, and the sound intensities, as measured in both arenas, were 94 db. (ref. 0.0002 dyne/sq.cm.) for the high level of sound and 87–88 db. for the low level. This softer noise served as the sound screen for masking incidental external noises. The loud noise was below the intensity which Morgan & Galambos (1942) found would cause audiogenic seizures with low frequencies. As will be seen later, there were no significant interactions, in any of the scores, between the noise and light variables and arena size. This suggests that the lack of complete uniformity in the light and sound fields had no profound influence on scores.

The carrying box, in which half the animals were shocked immediately before being tested in the open field, followed the design of Warner (1932). Each wall and half the floor constituted one electrode, and was made from one piece of aluminium, bent to shape. The two pieces were close enough together to force an animal to stand partly on each, yet with sufficient gap between the floor sections to prevent shorting caused by excrement. A Perspex lid was rigidly fixed at right angles to the top of one wall which swung outwards about pivots near the base of the box. This arrangement afforded a wide opening by which to insert and remove the animal, since both lid and one wall moved aside together. Through the feet of a rat standing in the box a current of predetermined value could be switched. This current was obtained from a 2 kV. transformer; it was limited by a 2-megohm series resistor and determined by a variable resistor in series with the transformer primary. Variations in the resistance of individual animals, which are known to be large, thus had little influence on the intensity of the shock. Indeed, decreasing the calibrating resistance of 220 K. to zero by shorting the electrode terminals altered the current by less than 11%.

(iii) *Subjects*

A group of albino rats were bred, as subjects for the experiment, from a remote Wistar strain, obtained originally from a dealer. It had been maintained by random mating in this laboratory for five generations. Twenty-four litters comprising 96 males and 110 females—an effective total of 192 with equal numbers of each sex—were reared under strictly standardized conditions from birth to the time of testing. An automatically controlled lighting system supplemented daylight in such a way that the colony room was in darkness only during the period 9.0 p.m. to 3.30 a.m. G.M.T. Handling was kept to the minimum. The litters were weaned at 21 days after birth, when the pups were sexed and caged in groups of five. At 50 days their ears were clipped under ether anaesthesia for further identification and they were caged thereafter in threes. The cages measure 9 by 9 by 8½ in. high and have solid metal backs, tops and sides. An *ad libitum* diet of rat cubes (MRC diet no. 41) and tap water was provided. All subjects were between 100 and 110 days old when experimentation on them began; at this time they had been handled only five times since birth. Thus, not only were the subjects experimentally naive at the beginning of testing, but also they had not undergone any taming or ‘gentling’.

(iv) *Procedure*

Food deprivation was commenced 5 days after the final cage allocations were made and 13 days before testing began. It was established by degrees, only reaching the final period of 2 hr. feeding per day after 9 days. This lengthy procedure was adopted in order to minimize weight loss and to ensure stability of hunger drive before the experiment began (Hall, Smith, Schnitzer & Hanford, 1953).

The 2 hr. feeding time followed the morning and the afternoon sessions, so that the food-deprived subjects were tested after a period ranging from 19½ to 21½ hr. without food. The other half of the subjects, who were not deprived of food, were subjected to disturbance comparable to that occasioned by replacing, and later removing, the food-hoppers from the cages of the animals actually deprived.

Animals were individually transported in the carrying box, described above, from their cages in the colony room to the open-field test where they were detained in the box for 15 sec. The pre-shock group were subjected to a current of 0.25 mA. a.c. through the feet during this time. This shock was always sufficient to evoke running movements and squeaking almost always. Defecation was less frequent. The combination of noise and light levels and arena size appropriate to the particular subject having been established, the rat was placed in the arena half-way between edge and centre, facing outwards. Each subject was left in the arena for 2 min. during which time ambulation was noted on a prepared plan of the arena floor, together with the incidence and amount of defecation. A urination score was not used because of the difficulty of scoring this variable objectively. Even without any attempt at quantification, and merely scoring urination as present or absent, it is almost impossible, with present techniques and especially in the large arena, to be sure that one spot of urine deposited somewhere has not been overlooked. The contrast with the ease of scoring defecation by counting the number of faeces deposited is noteworthy. Testing took place on five successive days. The order in which individuals were tested was not varied. The three replications of the experiment were begun at intervals of 1 week.

III. RESULTS

(i) *Scoring*

The defecation scores were analysed in two ways: first, by the frequency of defecation, that is, the number of days out of the five upon which any defecation occurred; and secondly, in terms of the amount of defecation, that is, the number of boluses deposited when defecation did occur. This was done in order to obtain a score which reflected the amount of defecation independent of its frequency. Ambulation, obtained as a raw score of the number of subdivisions marked on the floor which the subject entered, was converted into metres by multiplying by appropriate constants.

In each case a total or an average score for the 5 days was used in the analysis, because examination of the day-to-day totals showed no evidence of differences in trend attributable to treatment effects. But it is of interest to note that the data from all 192 subjects (irrespective of treatment), for the average daily defecation score, clearly shows the characteristic decline in magnitude with repeated daily exposure of the subjects to the open-field test. In contrast, the average daily ambulation score shows little change after an initial drop. It is clear that there is no pronounced day-by-day trend towards the increased ambulation to be expected if familiarity with the situation decreased a tendency to emotional 'freezing'.

(ii) *Litter groupings, age and time of testing*

Before proceeding further it is desirable to indicate how far it is possible to eliminate from consideration possible sources of bias in the results due to litter grouping, age and time of day of testing. A preliminary investigation of litter groupings was made by considering the results in the form of block totals. An analysis of variance for each of the three scores analysed (that is, defecation frequency, defecation amount and ambulation) showed a significant 'between-blocks' effect, as might be expected as a result of the differences in the experimental treatment imposed on their members. Yet, only those blocks in which most of the animals had been subjected to treatment combinations which included significant main effects (see later) reflected this circumstance by the extremeness of their scores. Moreover, there was no evidence, among those blocks representing parts of the same litter, of any systematic high or low scoring. It was concluded from this simple analysis that litter differences could be excluded from further consideration, and that the more elaborate treatment which this design allows (Yates, 1939) was not needed.

It will be recalled that age was confounded with replication in that there was a small, though highly significant, difference between the mean ages of the subjects at the start of the three replications—102, 105 and 108 days respectively. Yet, for none of the scores analysed was there a significant F -ratio for the replication variable. This shows that such small differences in age do not affect the responses to the open-field test, and permits us to pool the results of the three replications. In subsequent discussion of the results this has been done.

The mean score for the morning session was compared with that for the afternoon session for each of the three scores. In each case the difference was small and non-significant. Thus, time of day of testing did not affect the results.

(iii) *Main findings*

Table 1 shows the mean value for each of the two sub-groups ($n=96$) comprised in each of the six treatments. The results of the analyses of variance* performed on the data are also presented in summary form. Opposite each treatment the P value, showing the significance of the associated F -ratio, is given. The F -ratios were obtained by testing the

Table 1. *Mean scores for sub-groups and summary of results of analyses of variance*

Treatment	Sub-group	Defecation frequency			Defecation amount			Ambulation		
		Means (days)	Signif. of F -ratio	D.F.	Means (boluses)	Signif. of F -ratio	D.F.	Means (metres)	Signif. of F -ratio	D.F.
Sex	Males	3.8	$P<0.001$	1, 182	3.2	N.S.	1, 1	8.4	N.S.	1, 1
	Females	2.0			1.8			11.4		
Noise	Loud	3.4	$P<0.001$	1, 182	2.8	N.S.	1, 1	11.2	$P<0.001$	1, 173
	Soft	2.4			2.2			8.6		
Light	Bright	3.2	N.S.	1, 1	2.7	$P<0.05$	1, 182	10.4	N.S.	1, 1
	Dim	2.7			2.3			9.4		
Arena size	Large	2.7	N.S.	1, 182	2.4	N.S.	1, 182	12.5	N.S.	1, 1
	Small	3.1			2.5			7.3		
Food deprivation	Not hungry	3.1	N.S.	1, 182	2.6	N.S.	1, 182	9.9	N.S.	1, 173
	Hungry	2.7			2.4			9.9		
Pre-trial shock	Shocked	3.0	N.S.	1, 1	2.2	$P<0.01$	1, 182	9.3	N.S.	1, 1
	Not shocked	2.9			2.7			10.5		
Overall means		2.9			2.5			9.9		
Error variance estimate		2.4			1.2			14.6		

Figures in *italics* indicate differences in the non-predicted direction (see text).

mean sums of squares either against the remainder error variance estimate, with which the non-significant interactions had been pooled, or against the largest of the significant interactions found. Reference to the column giving the degrees of freedom for each F -ratio will show which kind of error term was used in any particular case. It will be noted that, in a design having two levels for each treatment, both main effects and interactions, of no matter what order, will have only one degree of freedom. The simple interactions which turned out to be significant in the analysis of defecation frequency and amount scores are shown in Tables 2 and 3. The results of the t -tests of the significance of the

Table 2. *Defecation frequency score: interaction between the light and pre-trial shock treatments*

Pre-trial shock		Light	
		Bright	Dim
Pre-trial shock	Shocked	3.5	2.4
	Not shocked	2.8	3.0

Figures in *italics* indicate pairs of mean scores differing in the non-predicted direction (see text).

* The usual tests of the assumptions of homogeneity of variance and normality of distribution underlying the analysis of variance are not readily applicable to this design. Examination of the data did not reveal striking departures from these assumptions; moreover, there is now evidence that the F -test is insensitive to moderate departures from them (Lindquist, 1953, p. 78f.)

differences between the various pairs of cell means are indicated in the appropriate positions between the pairs of scores. All levels of significance were obtained from a one-tail distribution, except in the case of sex difference (where no prediction had been made), and in those cases where a difference in the non-predicted direction was found. Italics are used in the tables to indicate these latter. Such differences are, of course, not significant in the predicted direction; accordingly, a two-tail distribution was used to evaluate the observed differences.*

Table 3. *Defecation amount scores: interaction between the noise and sex variables*

Sex		Noise		
		Loud		Soft
	Males	3.3	N.S.	3.1
		<i>P</i> < 0.001		<i>P</i> < 0.001
	Females	2.3	<i>P</i> < 0.001	1.2

The analysis of variance of the ambulation scores is complicated by a significant third-order interaction which cannot be conveniently tabulated. Study of this interaction shows that the light and pre-trial shock factors vary apparently unsystematically with respect to each other and to the other two factors involved, arena size and sex. Arena size has a consistent influence on ambulation scores in the non-predicted direction, that is, rats always ambulate more in the larger arena. This permits the application of a *t*-test to the difference between the means which yields the highly significant value of *P* < 0.001. The sex variable shows a similar effect; females ambulate very significantly more than males (*P* < 0.001). The simple interaction between these two variables is given in Table 4.

Table 4. *Ambulation scores: interaction between the arena size and sex variables*

Sex		Arena size		
		Large		Small
	Males	10.3	<i>P</i> < 0.001	6.4
		<i>P</i> < 0.001		<i>P</i> < 0.05
	Females	14.6	<i>P</i> < 0.001	8.2

Probabilities *in italics* indicate pairs of mean scores differing in the non-predicted direction (see text).

Several points arise from a consideration of these results. The sex difference which emerges from our data is unequivocal. Males defecate significantly more in both frequency and amount and ambulate significantly less than females. This finding illustrates the frequently reported negative correlation between defecation and activity scores in the open-field test; the emotional animal defecates more and ambulates less than the non-emotional. Another aspect of the sex difference is seen in the interaction Tables 3 and 4, each of which shows that females are more responsive to one of the situational variables.

Noise is the only other factor which produces an effect on scores comparable to sex in

* The writer is aware that this is a controversial procedure. Justification is claimed in this case because of the tentative nature of the predictions made. It may also be added that the use of a two-tail distribution throughout would not have altered materially the levels of significance reported, or the conclusions drawn from them.

size and consistency. This potent effect obscures their usual relationship, for, while noise in general evokes greater defecation, it also increases rather than decreases ambulation. Intensity of illumination is a less powerful factor in the production of emotional responses. The higher level has no consistent effect on ambulation, but does yield higher defecation amount scores, though at a low level of significance. It has a marked effect upon defecation frequency scores, but only if the animals have been previously shocked (Table 2). This suggests that the shock sensitizes subjects to the effects of light. Greater arena size is ineffective in provoking more defecation. The effect is confined to the ambulation scores where it is very marked. The effect of food deprivation is a very small one. This absence of significant effect shows defecation scores to be independent of differences in the amount of faecal material available which may have resulted from the food deprivation variable. Similarly, ambulation scores are independent of hunger drive.

On the other hand, both satiated and hungry rats defecate significantly less in amount if shocked first (Table 1). That is, a reduction in the amount of faecal material available, resulting from previous defecation elicited by pre-trial shocking, does, in fact, have an effect on defecation scores in the open-field test. This finding appears to conflict with the statement above regarding the independence of defecation from faecal bulk. But the apparent paradox may be resolved by considering the pre-trial shock as an independent emotionally stressful experience, and considering the defecation responses to it of the satiated and hungry rats separately. No significant difference is found; that is, in the shocking box, as in the open field itself, possible group differences in faecal bulk are unimportant. But this conclusion must be limited to the first stressful situation imposed on the animal. It can thus be seen that the effect of shock upon the open-field test defecation scores has been obscured by an artefact of the experiment in that shock elicited the same response as did the test itself, and that the lower defecation amount scores of the shocked group is a direct consequence of the previous stressful situation, one which the non-shocked group had avoided. The fact that the shock had no such general effect on frequency of defecation in the open field, but only on its amount, appears to confirm this view.

IV. DISCUSSION

To what extent do the striking and consistent sex differences noted represent a real difference in emotionality between the sexes? We can assume that the found difference in the amount of defecation score represents a difference in the weight of faecal boluses produced (Anderson & Anderson, 1938). But since male rats are heavier than females, the alternative explanation that the difference in amount of defecation merely reflects the sex differences in body weight must be considered (Hall, 1934). The present finding that males also show greater frequency of defecation, taken in conjunction with Billingslea's report (1940) of a near-zero correlation between this variable and body weight, suggests a genuine difference in emotionality. Also, the results of the open-field tests (given, in connexion with another experiment, to a group of twenty-four pre-pubescent rats aged 50 days) show no significant difference in defecation scores, while a weight difference, significant beyond the 1% level, was found. Apparently the sex difference in emotionality develops after the age of 50 days and before 100 days—the age of the subjects used in this experiment—along with the differentiation of sexual function, and independently of the already established sex difference in weight. Furthermore, the direct test of the relation

between the defecation amount scores and the body weight of the sixty-four animals in the third replication of the present study yielded correlations between body weight after testing and the amount of defecation of 0.2 and 0.3 for males and females respectively—low and hardly significant. In sum, therefore, it may be said that while it is not possible to reject completely an hypothesis attributing the difference in the amount of defecation found to a sex difference in body size, the available evidence points away from it. The decrease in defecation in the open field by female rats during periods of sexual receptivity (Anderson, 1940) is a possible contributory cause. Since the oestrous cycle in the female rat lasts 4–5 days, during the five consecutive days of testing all of our female subjects should have been on heat at least once.

These highly significant sex differences in emotional elimination are, therefore, a principal finding of this study. They differ from some previous reports (Farris & Yeakel, 1945; Bindra & Thompson, 1953). Turning to ambulatory activity, Thompson's (1953) finding that females are more active than males is confirmed at the same high level of significance. A greater reactivity of females to situational stimuli has also been observed. This was particularly evident in the defecation response to noise, but a similar effect of the larger arena on ambulation scores was found. All these considerations make it clear that work on individual differences in emotionality in rats should employ different criteria for each sex.

Evans & Hunt's results (1942), relating to the effects of light on defecation scores, are, in general, confirmed and extended to include reaction to noise. Within the limits of this experiment, greater intensity of such physical stimuli yields higher defecation scores. This confirmation of the quasi-lawful relationship between stimulation and the most important index of emotionality in the rat is encouraging—and the measure of experimental control over it, thus achieved, promises well for the future use of the test. Moreover, it supports the status of defecation as a valid measure of emotionality in that variations in putative emotional stimuli did, in fact, influence the measure in the expected direction. But similar expectations regarding the variable of pre-trial shock and arena size yielded no such results. The effect of shock was, in fact, in the reverse direction. However, this apparent discrepancy, as has already been pointed out, can be satisfactorily explained. The large arena did, however, fail to influence defecation in the expected direction. In fact, the trend of results was in the opposite sense; the smaller arena eliciting more, rather than less, defecation (see Table 1). This effect may well be bound up with the large influence it exerted upon ambulation scores. Be that as it may, the finding has practical importance—there is no need to use an arena as large as that introduced by Hall (1934). The justification for the current use of smaller arenas for testing rats (e.g. Thompson & Bindra, 1952; Watson, 1954) thus becomes explicit, and Lebo's argument (1953)—for abandoning the open-field test because it is cumbersome—loses point.

The ambulation score is of less value than defecation as an index of emotionality. The absence of a marked trend towards increased ambulation with repeated testing has already been noted. This is consistent with the findings of other workers (e.g. Berlyne, 1950). Both the larger arena and the louder noise caused an increase, rather than a decrease, in ambulation. In the former case, it is clear that we are dealing with an aspect of exploratory drive—a larger arena causing more ambulation. Montgomery's finding (1951) that the amount of exploration done by rats increased with the size of the maze, and is proportional to the total area, is comparable. In the case of noise, the possible influence

of an audiogenic seizure mechanism must not be overlooked. The louder noise was of sufficient intensity to provoke much activity, either directed towards escaping from its intensity, or because the high-frequency components of it were above the threshold for eliciting a motor response which could have led (though never did lead) to an audiogenic seizure. A practical implication seems to be that it is probably better to avoid such intense noise in routine testing, particularly in view of the greater convenience of the softer noise. These findings illustrate a difficulty, noted by Anderson (1940), regarding the argument for the validity of a particular response as an index of emotionality on the grounds that emotional stimuli increase that response. Obviously the possibility of any direct reflex action must be excluded, for, should such an effect be in the same direction as the expected emotional response, a spurious conclusion might follow. Fortunately in this investigation, the effect of noise on ambulation was in a direction suggestive of lower rather than higher, emotionality. And with respect to the conclusions reached regarding defecation, it is difficult to envisage any direct effect of either noise or light upon faecal evacuation.

The relative absence of any effects attributable to hunger are of considerable interest. Hall's report (1936*a*) of differences in emotional responses between hungry and satiated rats is not confirmed and, accordingly, his argument that adjustment to the open-field situation is a function of hunger drive is not supported. Regarding the effect of food deprivation upon ambulation scores, the literature contains similar results (Montgomery, 1952; Thompson, 1953). Taken in conjunction with the relation between arena size and ambulation, this finding that increased hunger drive (which is known to augment significantly general activity in cages) has no significant effect on ambulation, compels the conclusion that the activity which the open-field test measures is externally rather than internally stimulated. That is to say, we are dealing here not with general activity, increased by hunger, but with exploratory activity stimulated by novel environmental conditions. This view has already been advocated by Anderson (1938), who notes that the lack of correlation between open-field ambulation and activity, as measured in cages, supports the distinction. Further support may be derived from the conclusions of Montgomery (1953), Berlyne (1950) and Harlow (1953), all of whom describe behaviour apparently motivated by exploratory drive.

One further point is worthy of comment. The relative success, attained in this experiment, of obtaining measurable amounts of defecation confirms the view (Hunt & Otis, 1953) that the open-field test might function more satisfactorily when made more stressful, and contrasts with those studies in which the test, used under conventional conditions, has yielded little or no score. But the strict maintenance of 'experiential naïveté' (Christie, 1951) in the subjects may also have been important. The effect on test scores of varying such background or life-history factors, held constant in the present study, is under investigation.

To summarize the practical implications for the use of the open-field test: the results suggest that a small, brightly lit arena with a noise level at least loud enough to act as a sound screen will yield satisfactory results. It is immaterial whether or not the subjects are hungry, but their sex is important, as is the occurrence in their immediate past of a markedly stressful experience. Defecation is the emotionality index of choice, measured preferably by amount rather than frequency, since the former will yield, if necessary, a continuous distribution of scores from a single testing and is slightly more sensitive.

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THE ASSESSMENT OF ANXIETY AS AN INTERVENING VARIABLE IN THE DELINQUENT BEHAVIOUR OF M.D. SUBJECTS. GALVANIC SKIN RESPONSE AND LEG-PERSISTENCE INDICES

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Anxiety is accepted as a stimulus-producing response or learnable drive to which certain delinquent behaviour is conditioned by drive reduction, and the relationship between fear, anxiety and pain resistance is discussed. The leg-persistence test is regarded as a pain-resistance test and is shown to be correlated with galvanic skin response fear reactivity. Persistence scores are shown to vary amongst delinquents according to the hypothetical degree of stress engendered by their previous misbehaviour and current hospital confinement. High anxiety (low persistence) subjects are found to be more unstable than others; age and intelligence to have no bearing upon the scores, but psychotic involvement to alter the distributions markedly. The data are discussed in terms of behaviour theory and the Eysenck typology.

I. INTRODUCTION

Fear is currently accepted to be a learned response to pain as a primary stimulus, and to operate in future conditions as a stimulus-producing response, or learnable drive, to which overt behaviour is conditioned by drive reduction (Miller, 1951). Anxiety is defined as generalized fear so that the theoretical relationship between pain resistance and anxiety is apparent, i.e. the greater the anxiety level, the more marked is the overt response to pain, and the less able is the subject to withstand pain. Hence, a crude measure of anxiety is the length of time that a subject can withstand a painful task. (Vide the reports of Hall & Stride (1954) and Stengel, Oldham & Ehrenberg (1955), and their references, for discussion of pain perception and pain reactivity in abnormal states respectively. Relatively little is reported on pain *resistance*, *per se*.)

Anxiety (in some form) is widely regarded as an operative factor in determining delinquent behaviour, but the level of manifest anxiety is partly determined by the environment of the subject at the time of examination. Hence, one would expect more anxiety in the period between the arrest and the trial, than after the sentence, and so on, but at the purely empirical level this is not necessarily a reason why pain resistance should not be used in the psychological prediction of future delinquency in subjects of known delinquent history. Biochemical, pharmacological and pain perception data in support of the view that pain resistance can be regarded as an index of anxiety in delinquent mental defectives, and evidence showing the relationship between pain resistance and ratings of proneness to relapse and the prevalence of psychosomatic disorders, are given elsewhere (Tong & Pugmire, 1955). The current report is concerned with investigations into (a) the relationship of scores for induced fear and pain resistance, and (b) the varying distributions of pain resistance scores amongst delinquent subjects.

The general theoretical position is that (a) a large proportion of delinquent behaviour is a learned response to stress or anxiety; (b) the commission of an illegal or punishable act, although providing temporary anxiety reduction, introduces further anxiety or fear;

and (c) the possibility of future delinquency is a source of anxiety for some (or many) delinquents. For the sake of clarity the standard terminology of behaviour theory is used (Hull, 1943, 1952, etc.), with the modifications for 'stimulation' and 'response' suggested by Miller & Dollard (1941, p. 59) to cover human social behaviour. The stress reaction hypothesis of delinquent behaviour differs from that of Maier (1949) in so much that stress and anxiety are regarded as synonymous for this purpose, and as stimulus-producing responses.

II. THE PERSISTENCE TEST

The following results were obtained from the use of a Leg Persistence Test similar to that described by Eysenck (1947, 1952), Brady (1948), and Gordon, O'Connor & Tizard (1954). It was applied by the writer as a persistence test in a small-scale study of delinquent mental defectives (Tong, 1954), but following an analysis of subjective reports and specific investigation (Tong & Pugmire, 1955) it was decided that the test is more meaningful regarded as a pain-resistance test. It is well suited to a larger scale study owing to the simplicity of the instructions and the absence of painful connotations. It is also suitable for repeated retesting purposes as there is little or no practice effect, but testing conditions, instructions, etc., must be rigidly controlled.

The test, as used, requires the subject to sit on a chair and hold his right leg horizontally about 2 in. above the seat of a chair of similar height. The score is the time in seconds that elapses before the subject lowers his leg to the chair seat. The instructions are simply—'Hold your leg above the seat of the chair as long as you can'; the maximum score is set at 180 sec.; there is no encouragement or incentive and the patient is given no knowledge of his score. Subjects report a dull pain in the leg after about 20 sec. in the required position (although this point varies greatly), followed by generalized fatigue pain.

III. THE SUBJECTS

These are some 200 male patients at a State Hospital for mental defectives who demonstrate dangerous or violent (delinquent) propensities so that specialized care and supervision is deemed advisable. They were selected randomly from those subjects referred for psychological examination, and are probably weighted slightly with subjects of rather higher i.q. than the Hospital population as a whole. The disordered behaviour demonstrated by any one subject may have occurred in an institution for mental defectives, in society whilst certified as a mental defective or in society prior to certification.

The main classification of the subjects is into (a) those demonstrating no psychiatric or psychological test evidence of possible psychotic involvement, and (b) those who demonstrate such evidence. The psychotic subjects are dealt with in a separate section at the end of the paper; i.q. scores, ages, etc., are dealt with in relevant sections.

IV. LEG PERSISTENCE AND THE GALVANIC SKIN RESPONSE FOR FEAR

The G.S.R. recorder provides a means of observing a subject's reactivity to a fearful stimulus, but difficulty is encountered in applying a satisfactory unit of measurement, particularly when refined statistical methods are required (Woodworth & Schlosberg, 1954, p. 141). Most recent reports refer to the various limitations (Haggard, 1949;

Lacey & Siegel, 1949; Bassett & Ross Ashby, 1954; Venables, 1955), but a method similar to that of Paintal (1951) appeared applicable for the current study. The method requires the establishment of a 'maximum' G.S.R. to which other scores are related; the index obtained is independent of basal ohmic level (Elliott & Singer, 1953), and is speedily obtained.

(i) *Hypothesis*

If the leg-persistence test (as a pain-resistance test) is indicative of the degree of anxiety (or generalized fear), the leg score should be negatively related to the index of galvanic skin responsiveness for induced fear, i.e. low pain score implies a high G.S. fear response.

(ii) *Method*

A record of G.S. responses was obtained by means of a Theratronics Ltd. G.S.R. recorder, recording by means of a galvanometer needle which carries a high voltage and sparks to an earthed recording plate via teledeltos (sensitive) paper. Palmar electrodes were used dry, and gave resistance variations to just beyond 1 megohm.

The leg-persistence test was applied to the patient before attaching the palmar electrodes. The patient was seated in a comfortable chair and the G.S.R. record taken for a resting period of 5 min. to determine the type of reactivity—labile, stable or mixed (Mundy-Castle & McKiever, 1953). Fifteen neutral words (i.e. non-'emotional') were then read out at 15 sec. intervals, and the patient required to give an immediate single-word association response to each stimulus word; the word list was then followed by a rest period when the patient was asked to relax completely and close his eyes. During this final period, three unexpected fear stimuli were given at 30 sec intervals, the first being a brush across the eyelashes with cotton wool, the second a slight touch to the inner lobe of the ear, and the third a sharp 'bang' with a ruler on a piece of card, near to the ear. All deflexions were measured in deflexion units (d.u.), basal level was ignored and the mean score for the neutral words (N.G.S.R.) taken as an index of general basic reactivity; the response showing the greatest deflexion for fear was chosen as the G.S.R. for fear (G.S.R.F.).

(iii) *Results*

The index $\text{G.S.R.F./N.G.S.R.} \times 100$ (in d.u.) was taken as an index of relative reactivity to fear and regarded as independent of basal level, temperature, etc. The records of twenty-five randomly selected non-psychotic subjects, excluding labile records and those records showing marked persisting resistance changes during the course of the test, were scored for the index given. The range for the fear index is 250–10,000 and for persistence scores for the same subjects is 180–17; the rank order correlation coefficient (ρ) between the respective scores is -0.576 , which is significant at the 0.01 level of P . It is concluded that the data support the hypothesis as stated, and tend to substantiate the general hypothetical position given in the introduction to the paper.

V. CLASSIFICATION OF THE SUBJECTS

Earlier work (Tong, 1954) merely classified the subjects into various operational groups, i.e. sexual and non-sexual disorders, violent and non-violent disorders, stable and unstable subjects, etc. Following consideration of the theoretical system given earlier, it

was decided to apply a classification according to the degree of stress or anxiety expected to be present in a given subject in his present environment, in the light of his previously demonstrated stress reaction pattern, i.e. his form of delinquency. This classification should, hypothetically, result in varying group scores with a test which mirrors the degree of anxiety demonstrated by the subjects, e.g. with a pain-resistance test.

Classifying according to history carries disadvantages with those subjects whose stress reaction pattern has at times been variable in nature. An attempt to classify according to predominant pattern was not successful owing to the difficulty of determining the predominant pattern in most mixed cases. The final classification decided upon for testing against the persistence hypotheses, was as follows:

(a) *Homosexual*. Those subjects who have demonstrated sexual misbehaviour with other males, irrespective of other forms of misbehaviour they may also have shown. These subjects were deemed to be in a relatively stressful environment in the Hospital owing to the availability (although very limited) of their stress reaction pattern with its undesirable consequences.

(b) *Sexual*. Those subjects who have demonstrated sexual misbehaviour involving females but who have no homosexual histories, irrespective of other forms of misbehaviour they may have shown. These subjects were deemed to be in a relatively stress-free environment owing to their current inability to demonstrate their habitual stress reaction pattern with its stressful consequences.

Sturup (1953) discusses the anxiety shown by Scandinavian sex offenders, particularly the fear of committing further offences, and is able to support his point by giving evidence of the readiness of such offenders to agree to voluntary castration. That anxiety is an important element in the structure of most offenders is a commonplace observation, but the sex offender is faced with additional stress in the way of moral reprobation from the time of his arrest until long after his return to society. The futility of procedures intended to frighten or discomfort anxious subjects into an alteration in their behaviour is evidenced by recent experiments with anxiety as a secondary drive (Gordon & Berlyne, 1954). Tong (1955*a*) found evidence of greater emotional disturbance in sex offenders in prison than was the case for other offenders, and Kay (1954) reported similar results with frustration experiments with young offenders in juvenile institutions. The vitally important aspect of specialized hospital handling of such individuals (as against punishment) is that a more empathetic attitude to their failings prevails for the staff, and for this reason, together with the removal of the fear of further offences, the sex offenders can be expected to adjust well in a protected environment.

(c) *Violent*. Those subjects who have not demonstrated homosexual or sexual misbehaviour, but who have shown violent or aggressive traits of a serious nature. For these patients confinement creates daily opportunity for their habitual stress reaction pattern, with the inevitable consequences, and confinement and discipline is deemed to be relatively stressful.

(d) *Abscond*. Those subjects who have not demonstrated homosexual, sexual or violent features, but who have repeatedly escaped from Institutions, Approved Schools, Prisons, etc., and committed various forms of crime not covered by the previous headings, e.g. housebreaking, larceny, arson, etc. Institutional confinement is stressful for these patients, almost by definition.

Table 1 gives the numbers of patients falling into these four categories, which are not

in hierarchical (hypothetical) stress order, with slight details of their disorders. It will be seen that the homologous offender is fairly common in the sample, and assuming that the disordered behaviour is a conditioned response to stress one must also postulate the existence of a hierarchical habit family of conditioned stress reactions in the case of the subjects with mixed histories. The Abscond group is composed almost entirely of homologous offenders, but this is partly due to the classification system adopted; in this group, however, the offences committed by any one subject are often markedly circumscribed, and with the housebreaker, for example, one finds that a given subject will select similar houses and apply almost identical modes of entry on each occasion. The sex reaction pattern is not so simple, however, and the demonstration of the particular behaviour pattern is not only dependent upon the availability of cues (i.e. the type of female liable to elicit the response, e.g. young girls) but also by optimal hormonal requirements.

Table 1. *Showing the types of offenders falling into the four disorder groups*

Classification according to offences	Total no. of subjects	Total no. of subjects of homologous history	Nos. of subjects who also have histories of offences which are		
			Sexual	Violent	Abscond
Homosexual ($N=30$): Intercourse	24	2	5	14	10
Other male indecency	6	3	1	2	1
Sexual ($N=40$): Sex attack and murder	2	2	—	—	—
Rape with violence	8	3	—	4	1
Statutory rape	10	3	—	4	3
Other heterosexual offences	20	11	—	5	4
Violent ($N=40$): Murder and attempted murder	5	4	—	—	1
Violent attacks	30	11	—	—	19
Robbery with violence	5	5	—	—	—
Abscond ($N=40$): With larceny	35	35	—	—	—
With arson or damage	5	5	—	—	—
	150				

VI. VARIATIONS IN PERSISTENCE SCORES

(i) *Hypothesis*. Assuming that pain resistance is inversely related to the degree of current anxiety, the groups described earlier should produce differing scores for persistence, dependent upon the degree of stress to which the types of patient are subjected, i.e. relatively high scores for the Sexual group, and lower scores for the Homosexual, Violent and Abscond groups.

(ii) *Results*. Table 2 gives the means and variances of the leg-persistence test scores for the various groups, and the data support the hypothesis as stated. Analysis of variance gives a value for F of 6.954, which is significant at the 0.01 level of P .

Table 2. *Means and variances of the four disorder types classified according to the hypothetical degree of current stress*

	N	Persistence score	
		Mean	Variance
Homosexual group	30	82.94	2421
Sexual group	40	108.32	2280
Violence group	40	76.4	1742
Abscond group	40	66.6	1308

Intergroup F -tests result in significant ratios for comparisons of the variances of the Sexual/Abscond groups ($F=1.657$) and Homosexual/Abscond groups ($F=1.729$), both significant at the 0.05 level of P . Intergroup t -tests applied where the variance ratios are not significant give the following results:

Sexual <i>v.</i> Homosexual	$t=2.144$, signif. at 0.05 level
Sexual <i>v.</i> Violent	$t=3.256$, signif. at 0.01 level
Violent <i>v.</i> Abscond	$t=1.16$, not significant
Violent <i>v.</i> Homosexual	$t=0.612$, not significant

Fig. 1 gives the distributions of the scores for the four groups.

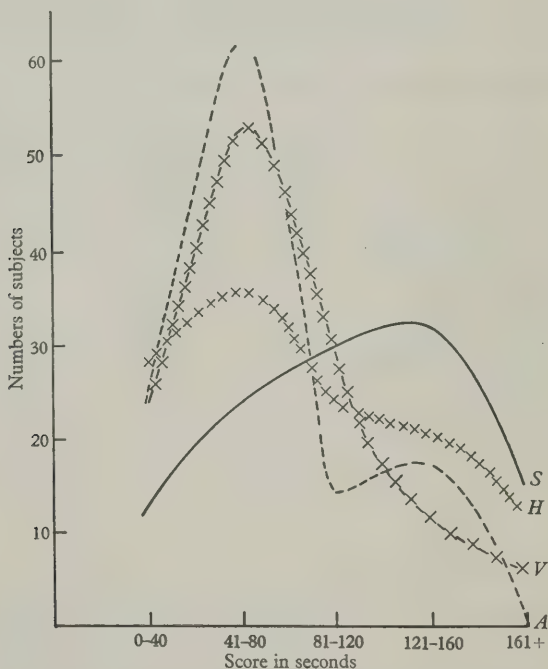


Fig. 1. Showing the distribution of persistence test scores for the four behaviour disorder groups. For each group N is prorated to the equivalent of 120. (S refers to the Sexual group; H , Homosex; V , Violent; and A , Abscond, as defined in the text.)

(iii) Effect of age

Age is a factor of some importance in tests involving pain *perception* (Hall & Stride, 1954), and the groups used for the current study give different mean scores for age, in years, as follows: Homosex: mean 29.5, variance 62.4; Sexual: mean 30.7, variance 59.7; Violent: mean 24.2, variance 37.8; Abscond: mean 24.6, variance 38.2. In the earlier study comparing Sexual (including Homosexual) with Non-sexual subjects, the persistence score differences reflected the age differences, and it appeared possible that the persistence score was not therefore a fundamental personality item. The greater differentiation of the current study does not support the view that age is a determinant. The Sexual and Homosexual groups are of similar ages yet give dissimilar persistence scores. Further,

rank-order correlations of age and persistence for each of the groups gives the following, not significant, values for ρ : Homosex, 0.02; Sexual, 0.17; Violent, -0.12; Abscond, 0.19.

Finally, twenty subjects whose ages were between 15 and 30 years were selected randomly from the Sexual group, and a further twenty subjects of the same age range selected from the Abscond group, i.e. the two extreme groups. The mean persistence score for these two groups is 107.4 and 62.8 respectively; these scores are similar to those given in Table 2, and do not indicate that age determines the differences in score in that table.

(iv) *Effect of intelligence*

Gordon *et al.* (1954) report a low but positive correlation coefficient for I.Q. and leg-persistence score with twenty-seven imbeciles, but no such relationship was apparent in a study of higher grade defectives (Tong, 1954).

In the interpretation of studies involving mental defectives above imbecile level, care must be taken not to confuse the medical/legal notion of mental defect with the psychological/statistical notion of low intelligence. Particularly where behaviour disorders are concerned, the term 'feble-minded' does not necessarily, in practice, imply low intelligence (Memorandum, 1947; cf. Woodward, 1955, and Milner, 1949).

The Vocabulary score of the Wechsler scale (1944) carries the research disadvantages of the full score to a lesser degree and in fact correlates highly with the full I.Q. (Pichot, 1949). It is applied here as a measure of the attainment level, and the mean scores and variances of the four groups are as follows: Sex: mean 13.47, variance 25.2; Homosexual: 16.5, 24.8; Violent: 12.75, 15.2; Abscond: 14.0, 20.2. Rank-order correlations (ρ) between persistence score and vocabulary score for each group are: Homosex, 0.275; Sexual, -0.406; Violent, 0.075; Abscond, 0.203. It is suggested that the negative correlation shown by the Sexual group (significant at the 0.05 level of P) is an artifact of the sample selection.

Raven Matrices test scores were used as indices of intellectual potential for twenty-five randomly selected subjects comprising four Homosex, four Sex, ten Violent and seven Abscond. The range for the Matrices score was 8-47, mean 26.16, variance 154.9. A rank-order correlation between Matrices score and Persistence gave a value for ρ of -0.074. It is concluded that neither intellectual attainment nor ability are determinants of the persistence score.

VII. HOSPITAL STABILITY

(i) *Hypothesis.* Those subjects who find the Hospital environment to be stressful (irrespective of disorder type) will react more obviously and will therefore be considered to be more unstable and will require more nursing attention than is the case for those who adjust well to the protected environment.

(ii) *Method.* (a) The leg-persistence test was applied as an index of current anxiety.

(b) The names of the 150 subjects discussed above were listed and the lists handed to three senior nursing staff of a ward, henceforth referred to as the judges. Independently, each judge was asked to select those subjects he knew sufficiently well to make a stability assessment on the basis of behaviour in the ward. The names of the selected subjects known to all the judges ($N=97$) were then listed and the judges asked to rate, independently, each subject as 'stable' or 'unstable'.

(iii) *Results.* Forty-eight subjects were rated in the same direction by all three judges,

twenty-eight being 'unstable' and twenty being 'stable'. The other forty-nine subjects were rated 'stable' by two judges and 'unstable' by one judge ($N=23$), or 'unstable' by two judges and 'stable' by one judge ($N=26$).

The mean persistence scores and variances for these four groups are given in Table 3. It will be seen that the means are in descending order corresponding to the degree of certainty (concerning stability) shown by the three judges. Analysis of variance results in a value for F of 4.731 which is significant at the 0.01 level of P (Kempthorne, 1952), and supports the hypothesis as stated.

Table 3. *Means and variances of the persistence scores of the four groups determined by the stability ratings*

Group	N	Mean	Variance
Rated stable by 3 judges	20	105.45	2089
Rated stable by 2 judges	23	90.24	2087
Rated unstable by 2 judges	26	86.14	2354
Rated unstable by 3 judges	28	58.56	1266

Such results, of course, may have reflected a tendency on the part of the judges to select sex offenders for the 'stable' group, as these patients have been shown earlier to be of higher persistence than the other groups. There is a slight tendency in this direction as is indicated by a χ^2 analysis, for which the contingency table is given below. The value for χ^2 is 3.34, which is not significant:

	Stable	Unstable	Total
Sex offenders	14	9	23
Other offenders	29	45	74
	43	54	97

(iv) *Intra-group stability rating.* An important theoretical and practical point is to determine the relationship of persistence and stability for each disorder type. In other words, does a subject's pain resistance score mean the same in stability terms irrespective of the disorder type, or do similar scores carry different meanings for different types of patient? Research into this point is proceeding with more specific tests, but there is evidence to show, for example, with the leg-persistence test that a score below a certain point—about 60 sec.—corresponds with instability and poor prognosis in the vast majority of cases, but for higher scores different confidence limits must be set up for the different types of disorder.

The ninety-seven patients rated for stability can be classified according to disorder type, although the numbers are small, and cross-classified according to whether they were rated as 'stable' (by two or three judges) or 'unstable' (by two or three judges). The mean persistence scores are as follows:

Stable: Homosex 86.6; Sex 141.4; Violent 89.0; Abscond 70.15.

Unstable: Homosex 63.37; Sex 108.2; Violent 69.7; Abscond 59.9.

The numbers of the subjects falling into each group are respectively: Stable: Homosex 10; Sex 14; Violent 6; Abscond 13; and Unstable: Homosex 16; Sex 9; Violent 16; Abscond 13. For this type of study the numbers are too small to be worthy of detailed statistical analysis or interpretation. It will be seen, however, that for each disorder group the Unstable score is lower than the Stable score, but, in addition, the Unstable persistence score for the Sexual group is higher than the Stable score of any other group.

VIII. PERSISTENCE SCORE AND SUBJECTIVE RATING OF FATIGUE/PAIN

Interesting results concerning the patient's own assessment of his fatigue point compared with his objective fatigue point are reported by Hemphill, Hall & Crookes (1952) in respect of anxiety states, hysterics and depressives. In this research a standard finger ergograph was used as the fatigue task and the patient was asked to indicate at which stage he felt tired and then to carry on as long as possible. This technique was found to be applicable to the subjects under discussion in the current paper, and consequently an assessment of the patient's subjective opinion of his fatigue/pain level was obtained by asking the question 'Do you ever feel tired or aching, or complain of being tired or aching?' before applying the leg-persistence test. The patient's answer would be classified under one of two headings: (a) complaint of tiredness or aching (an affirmative answer or any qualification), or (b) no complaint of tiredness or aching (a definite negative answer).

The hypothesis to be examined is that the leg-persistence score is related to a general awareness of fatigue/pain in other, non-specific, situations. The persistence scores of thirty-four patients who gave answers falling under type (a) above were compared with the scores of thirty-four patients whose answers indicated no awareness of fatigue/pain, type (b). A $2 \times 2 \chi^2$ test was applied to the data with the persistence score reduced to a dichotomous variable at the median. The value of χ^2 was found to be 4.974, Yates's correction 4.026, which is significant at the 0.05 level of P . The actual contingency table is as follows, and indicates that there is a relationship with the persistence scores and general non specific awareness of pain and fatigue:

	Persistence score		
	Below 91	Above 90	Total
'Complaints of tiredness or aching group'	25	9	34
'No complaint of tiredness, etc.'	16	18	34
	41	27	68

IX. PSYCHOTIC INVOLVEMENT

The problem of the offender who demonstrates psychotic dysfunction is relatively rare, and even in specialized hospitals the numbers of subjects falling into the various diagnostic categories is usually insufficient to warrant sound conclusions from group studies, particularly if it is desired to cross-classify according to behaviour disorder type. It is generally accepted that the possibility of psychotic involvement must be explored in the early stages of a psychological examination.

Leg-persistence test scores for fifty-eight subjects demonstrating psychological test evidence or psychiatric evidence of psychosis are available. The psychological tests used were Klopfer & Kelley's (1946) system for the Rorschach material, the Wechsler Bellevue scale and the Wechsler Memory scale, the Barrow Hospital Sorting Test (modified) (Hall, 1951, 1952; Tong, 1955*b*), and various conceptual and perceptual tasks. The diagnoses, or diagnostic indicators on tests, suggest: schizophrenia, 28 subjects; post-encephalitis lethargica syndrome, 13; epilepsy, 9; intracranial organic pathological state (undifferentiated), 4; paranoides dementia, 3; manic depressive, 1. Classification according to behaviour shows 9 Homosex; 12 Sexual; and 37 Violent. The mean persistence scores for the behaviour groups are Homosex 35.0; Sexual 69.9; Violent 105.5. It will be seen that the relative position of the three groups is different from that for the non-psychotic

subjects given in Table 2. However, inspection of cross-classification tables reveals that, for these subjects at least, the nature of the psychosis may be the determinant of the persistence score. Of the thirteen post-encephalitic subjects only one scores above 90, whereas only one of the nine epileptics scores below 100. Further for these two groups, eight of the nine Homosex are post-encephalitic, and all the epileptics fall into the Violent behaviour group.

Of the thirty-eight schizophrenic subjects, one is classifiable as Homosex, 9 as Sexual, and 37 Violent, with mean scores of 72.2 and 109.3 for the two latter groups—again a reversal of the position for the non-psychotic subjects. The distribution of the scores for the schizophrenia group is markedly bimodal, with the peaks at below score 50 and above score 180.

The problem of pain resistance in psychotic states may not be one of learning theory but of neural facilitation, but the data at least support the view that early differentiation in diagnosis is advisable, and that any technological conclusions drawn from the early figures for non-psychotics do not necessarily apply where there is gross dysfunction.

X. DISCUSSION

The data given in the paper tend to support the loose extension of academic theory forming the introduction, but in addition, and of equal importance, open up many problems which are approachable by systematic study. At this stage, allowing for the validity of the theory, discussion concerning the respective values of molar or molecular levels is of slight utility only, but it is suggested that further research will require molecular structure to account satisfactorily for the position of 'anxiety', 'fear' and 'pain'. Knopfmacher (1953*a, b*) and Jones (1954) found difficulty concerning the role of 'stress' or 'frustration' as determined by Maier (1949) and suggested the need for physiological derivation after the lines of Cannon (1932) and, later, Selye (1950). 'Anxiety' is a concept similar to 'stress' and may be better contained in a theoretical system containing physiological variables, possibly similar to that of Hebb (1949).

Concerning the role of anxiety, the experimental data relating the scores of the G.S.R. for fear and the persistence scores are of interest in the light of Eysenck's theoretical model, particularly a recent paper (1955). In the latter article Eysenck reviews the anxiety (as a secondary drive) experiments of Taylor (1951) and Spence & Taylor (1951), etc., where the experimental situation involved the use of the M.M.P.I. derivative—the Taylor Anxiety Scale. The point is made that this test correlates highly with the Maudesley Medical Questionnaire which is highly loaded with the Neuroticism factor of Eysenck's model, and it is suggested that the anxiety indices are therefore measures of Neuroticism. Leg-persistence test data are used by Eysenck (1947, 1952) for analysis of the typology, and the question arises whether the test data are better regarded as indices of fear or anxiety in the behaviour theory structure rather than in the typological sense that they have been applied.

The same paper by Eysenck (1955) raises the question of the 'psychopath' in his framework. The subjects discussed earlier certainly fit the description of 'psychopath' given by Scott (1954). Eysenck's typology associates, to some extent, psychopaths, criminals and hysterics, and he quotes their characteristics of 'lying, stealing, sexual delinquency, avoidance of responsibility, etc.' The data given earlier, taken at the

empirical level alone, at least suggest that the group 'psychopath' or 'criminal' is probably not unitary but spread along the dysthymic-hysteric continuum, possibly in relation to the type of behaviour disorder. It is important to note concerning researches comparing anxiety states, hysteria and psychopathy, when psychiatric diagnoses are involved, that the terms are at two different levels of semantics. Anxiety state and hysteria are usually (in the psychiatric sense) based upon the intervening variables of some form of analytical type theory, whereas psychopathy is usually descriptive of behaviour alone.

The general problem of pain resistance in theoretical structure is discussed elsewhere (Tong & Pugmire, 1955) at a molecular level, but in the comparison of studies involving cutaneous pain (Hall, 1953) and dull pain such as that of muscular tension, the duality of pain must be considered (Morgan & Stellar, 1950).

For delinquency theory the main points need corroboration with non M.D. subjects, but the following features appear fairly clear:

(a) Due consideration should be made of the environment of the subject at the time of the examination. For example, is a comparison of free normals with imprisoned offenders a valid study for the differentiation of personality variables by means of a test (Porteus, 1954)?

(b) Statistical studies which regard offenders as forming a homogeneous group should be interpreted with care as the nature of the offence may be an important variable.

(c) The psychotic offender may require separate consideration with any technique involving prediction of stability.

(d) The importance of anxiety is emphasized by experiment, and for therapeutic purposes anxiety may be better regarded as fundamental rather than symptomatic. Following other researches with anxiety, punitive measures are unlikely to succeed in extinguishing unwanted behaviour, at least with a large proportion of subjects of the type discussed.

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NEUROTICISM IN CHILDREN

I. AN INVESTIGATION OF NORMAL AND NEUROTIC GROUP DIFFERENCES

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Three groups of children aged 11–15 years, equated on sex, age and intelligence, were given a battery of neuroticism questionnaires and objective performance tests. The experiment was designed to throw light on the effects of the testing situation upon test scores. The results indicate that all the tests, questionnaire or objective performance alike, are to some extent affected by the testing situation, the questionnaires being so much affected as to be virtually useless in discriminating between normal and neurotic groups when the testing situation is more strictly controlled. The objective performance tests still appear to behave in the manner expected of them in spite of the additional controls.

I. INTRODUCTION AND PROBLEM

Early work on the dimensional analysis of personality by Eysenck (1947, 1952) and his colleagues has indicated the existence of at least three dimensions which are relatively independent of one another. Besides intelligence they are—Neuroticism, Introversion-Extraversion, and Psychoticism. It is with Neuroticism that the present experiment is concerned.

Work with adult neurotics suggests that in many cases these patients have a long history of emotional disorders reaching back into their childhood. Would it be possible, therefore, to identify and measure the same factors of neuroticism or emotional instability in children as well as adults, and would the same types of tests be suitable for this measurement? An early attempt to provide answers to these questions was made by Himmelweit & Petrie (1951); the outcome of their work was strongly positive. Even more positive were the results reported by Connor (1952) who used many of the measures originally designed by Himmelweit & Petrie, and whose work was carried out under the guidance of Prof. P. E. Vernon. Similarly, Eysenck & Prell (1951) arrived at a favourable conclusion. It seemed worth while to repeat these studies on other groups and also to introduce certain controls into the experiment which had not been employed before, but which appeared essential before any definitive conclusions could be reached.

The main innovation in the area of additional controls was connected with the problem of eliminating the possible effects of certain factors of set and attitude which might influence results in a variety of ways. The main experimental design consisted of comparing the test scores of normal and neurotic children, equated on other relevant variables. Predictions were made in each case as to the expected direction of the scores in line with theories derived from work with older subjects. Highly significant differences, such as those obtained by Himmelweit & Petrie, by Connor, and by Eysenck & Prell might reasonably be interpreted as a reflexion of the greater emotional instability of the group of neurotic children. However, in all preceding investigations, neurotic children had been tested in the clinic, which to them undoubtedly was associated with detailed discussions

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of their symptoms, with highly emotional reactions, and with attitudes and behaviour patterns of a neurotic character. Normal children had usually been tested in their own schools, but even when tested in the clinic there could be no such disturbing associations for them as there had been for the neurotic children. The possibility, therefore, had to be placed that the differentiation between normal and neurotic children might be an artefact caused by the determining tendencies or 'sets' arising from the place where testing occurred.

To overcome this difficulty, a plan was adopted which first arose in discussions with Mr D. Prell. In brief it was decided to equip a large caravan as a mobile laboratory, to take this caravan to the schools to be sampled, and to carry out all testing on the precincts of the schools in question. By a procedure to be explained later, neurotic children and normal controls were selected in such a way that their selection appeared to be due to chance. In this way it was hoped to eliminate the association for the neurotic child of his symptoms and troubles with the testing, there being no apparent connexion between the caravan and the clinic.

A final check on the effects of set on test scores was provided by testing a further group of children, all of whom were receiving treatment at the Maudsley Children's Out-Patient Department, but this time the testing was carried out in the clinic. If the place of testing and its associated attitudes was important, this could be demonstrated by comparing the test scores of this group with those of the previous group of emotionally disturbed children tested in their schools.

II. THE EXPERIMENT

A. *Subjects*

Three groups of children were involved in the experimental design.

The first group comprised thirty-four children who were receiving psychiatric treatment for emotional disturbances. These children were between 11 and 15 years of age, with a mean age of 12 years 6 months. The second group comprised 106 normal children to serve as controls, and a third group was made up of a further twenty-eight children all receiving psychiatric out-patient treatment. All these groups were equated on sex, age, and intelligence, and there were no reasons for suspecting that the first and third groups differed in their degree of maladjustment.

B. *Tests*

The main criterion in the selection of tests was that they had in previous studies been shown to discriminate between normal and neurotic groups. These tests are too well known to warrant detailed descriptions of them here. The verbal tests are identical with those used by Connor (1952), and brief descriptions of them appear in a paper by Thorpe (1955). Adequate descriptions of most of the objective performance tests will be found in the two books by Eysenck (1947, 1952). Three new measures of neuroticism were introduced—the Body Sway number of reversals score, which was a straightforward count of the number of times the subject's movement in the forward-backward plane changed direction during the Body Sway test, and two Reaction Time scores, obtained from the test fully described by Rodnick & Shakow (1940). Five potential measures of psychoticism were also included.

Verbal tests

Thurstone's Primary Mental Abilities (11-17 years)
 Sentence Completion
 Annoyances
 Ways to be Different
 Medical Questionnaire
 Worries and Anxieties
 Interests
 Sociometric

Performance tests

Manual Dexterity (sum of M N O and P scores)
 Track Tracer (amount of time spent in error in $\frac{1}{100}$ sec.)
 Leg Persistence (time in seconds)
 Dynamometer Persistence (time in seconds)
 Static Ataxia (sway in inches)
 Body Sway Suggestibility (sway in inches)
 Body Sway—number of reversals
 Autokinetic Movement (in inches)
 Rail Walk (in feet)
 Tapping—maximum tempo (number of taps)
 Reaction Time—regular preparatory interval (in $\frac{1}{6}$ sec.)
 Reaction Time—irregular preparatory interval (in $\frac{1}{100}$ sec.)
 Luria—left-hand disturbance
 Dark Adaptation (in grades)
 Mirror Drawing (number of errors)

The following tests of psychoticism were also included:

Three circles (time in seconds)
 Three squares (time in seconds)
 Waves—amplitude (in millimetres)
 Waves—wave length (in millimetres)
 Tapping—normal tempo (number of taps)

C. Method

The names of thirty-four children between 11 and 15 years of age were selected from the records of the Children's Departments at the Maudsley and St George's Hospitals. All the children were attending their schools in the normal way, but were considered to be sufficiently disturbed emotionally to need out-patient psychiatric treatment. Care was taken that none of these children suffered from organic complications, possible psychotic disturbances, or complaints of a physical nature. These thirty-four children were, with the permission of the L.C.C. and the school head teachers, tested on the premises of their own schools.

In order to obtain the normal control group, three children were chosen from the school class of each of the thirty-four neurotic children, who most closely resembled him in sex, age and intelligence. This was made possible by administering the intelligence test to the

whole class, and scoring it during the subsequent testing session with the verbal tests. In this way a control group of 106 children equated on sex, age and intelligence with the group of thirty-four neurotics was formed. The children were not informed, however, of the selection procedure. They were told that some of their names would be drawn out of a hat and that these pupils would be doing some other tests (the performance tests) individually in the caravan. By following this procedure it was hoped to eliminate the associations for the neurotic child between his symptoms and the testing.

The third group in the experimental design comprised twenty-eight children, all attending the Maudsley Children's Out-Patient Department for emotional difficulties, and equated for sex, age and intelligence with the first group of neurotics. These children were tested individually on both the verbal and the performance tests, but as in most cases a parent was waiting for the child the test battery had to be somewhat reduced.

In summary, three groups of children, equated on sex, age and intelligence, were involved. The first (neurotic) group was tested in the schools, the second (control) group was also tested in the schools, and the third (neurotic) group tested in the clinic. There were no reasons for supposing that the third group was any more or any less neurotic than the first.

III. RESULTS

The means and variances of the test scores for each of the three groups are given in Table 1.

Table 1

Variable	Expected direction of score indicating neuroticism	Neurotic school tested ($N=34$)		Control school tested ($N=106$)		Neurotic clinic tested ($N=28$)		Critical ratio, normal and neurotic school tested*
		Mean	Variance	Mean	Variance	Mean	Variance	
1. Age in years	—	12.500	1.217	12.475	1.366	12.833	1.191	N.S.†
2. Total P.M.A. score	—	94.917	875.415	92.121	981.000	90.032	1421.312	N.S.
3. Sentence completion	High	3.455	4.4	3.623	4.6	5.285	5.1	0.406
4. Annoyances	High	24.079	96.6	25.399	63.5	24.607	82.3	0.712
5. Ways to be different	High	20.992	128.7	25.004	167.2	23.750	143.9	1.787
6. Medical questionnaire	High	8.879	16.3	9.315	17.4	10.500	20.3	0.233
7. Worries and anxieties	High	12.172	35.5	11.872	36.0	13.964	38.6	0.256
8. Interests	High	15.700	36.5	15.445	33.1	14.607	25.9	0.216
9. Sociometric	Low	0.204	0.081	0.312	0.088	N.G.‡	N.G.	2.038
10. Manual dexterity	Low	201.147	559.5	203.264	361.7	196.880	411.6	0.475
11. Track tracer	High	116.895	5078.4	132.973	8485.7	N.G.	N.G.	1.355
12. Leg persistence	Low	30.547	464.5	41.586	787.6	29.793	422.9	2.406
13. Dynamometer persistence	Low	30.517	1374.1	46.808	1918.9	22.596	850.0	2.131
14. Static ataxia	High	0.735	0.451	0.636	0.378	0.570	0.512	0.847
15. Body sway	High	2.183	6.3	2.293	5.9	2.527	6.2	0.224
16. Body sway reversals	High	8.937	132.4	7.896	125.4	N.G.	N.G.	0.463
17. Autokinetic	High	23.844	442.1	20.473	565.7	N.G.	N.G.	0.787
18. Rail walk	Low	97.109	304.0	91.500	476.6	N.G.	N.G.	1.530
19. Tapping maximum	Low	59.006	146.1	56.353	86.0	N.G.	N.G.	1.174
20. Reaction time—regular	High	31.569	88.0	29.863	42.9	33.629	258.2	1.116
21. Reaction time—irregular	High	34.100	59.5	33.360	68.9	36.518	297.8	0.516
22. Luria	High	6.027	47.3	5.649	36.9	N.G.	N.G.	0.286
23. Dark adaptation	High	22.668	20.5	21.799	19.2	N.G.	N.G.	0.984
24. Mirror drawing	High	61.916	7128.4	51.174	5375.9	62.504	1287.6	0.666
25. Three circles	—	16.993	148.4	17.108	83.3	N.G.	N.G.	0.051
26. Three squares	—	24.137	191.9	24.778	157.8	N.G.	N.G.	0.240
27. Waves—amplitude	—	20.698	11.9	19.646	12.6	N.G.	N.G.	1.538
28. Waves—wave-length	—	92.755	387.1	89.418	457.6	N.G.	N.G.	0.842
29. Tapping—normal	—	34.221	199.4	35.767	169.2	N.G.	N.G.	0.566

* A CR of 1.65 or more is required for significance at or beyond the 5% level.

† N.S., not significant.

‡ N.G., not given.

IV. DISCUSSION OF RESULTS

Discussion may first be centred around the test performance of the neurotic and normal groups tested in the schools. It will be seen from the table that the seven verbal tests (numbered 3 to 9) fare rather badly on the whole in terms of the predictions that were made for them, only three of them behaving as predicted. Moreover, the difference between the groups on the Ways to be Different test would have been statistically acceptable had the *opposite* prediction been made. In view of the results of both Himmelweit and of Connor, in which the same or similar tests were shown to discriminate in the predicted direction at high levels of statistical confidence between neurotic and normal groups, the present finding is rather serious in its import. At the same time a considerable weight of evidence suggests, as is now well known, that tests of this type are notoriously unreliable owing to the effects of the testing situation, the sex of examiner, the motivation of the subjects, and so on. Meehl & Hathaway (1946) are two of the many writers who have drawn attention to this. Faw (1948) found changes in Bernreuter responses according to the testing situation, over 15% of the subjects changing more than fifty percentile points from one situation to another. Eysenck, too, is acutely aware of this difficulty which leads him to place more reliance upon tests of the objective performance type in the investigation of all basic personality dimensions. He writes: '(Performance tests) come nearest to the ideal of objective empirical investigation; they suffer less from the evils of voluntary distortion. . . . Our hope of building up a true science of personality rests on the development of this type of test more than on any other' (1950*a*, p. 214). It is to this group of tests that we must now turn.

In the results for the fifteen objective performance tests of neuroticism administered individually in the caravan, all except four behave in the manner expected of them. These exceptions are the Track Tracer, Body Sway, the Rail Walk, and the Tapping (maximum tempo) tests. Of these the results for the Body Sway test are perhaps the most serious, this test having previously shown good discrimination between groups of normals and neurotics, whether adults or children. A detailed discussion of this result will follow later. Of the eleven individual tests behaving as predicted, only two (the two tests of persistence) show differences between the normal and neurotic groups large enough to be statistically significant.

The implications of the results of comparing the performances of neurotic and normal children tested in their schools are that there is little hope of using group tests of the questionnaire variety in this way for the screening of such groups. Most reliance can at this stage be placed on the sociometric test (Thorpe, 1953) for which a significant tendency for neurotic children to be less popular in terms of this test than the normal controls is demonstrated. For the remainder of the verbal tests, whether considered individually or together, their screening efficiency is virtually zero. Such a conclusion would not, however, obtain for the individual performance tests, eleven of the fifteen showing the differences between the groups which were predicted. Even though only two of these eleven tests showed statistically significant differences, that the overall results are not due to chance is indicated by a χ^2 of 3.3 (1 D.F.) on a hypothesis of chance success.* This value of χ^2 approaches significance at the 5% level and it may be taken to mean that the

* Though this test is not strictly applicable as the tests are not independent, this trend should be compared with that for the verbal tests where less than half of them behaved as predicted.

individual tests on the whole have given results in line with previous findings, even though, in the present case, such factors as the attitudes and 'sets' of the subjects have been thoroughly controlled.

The psychoticism test performances of the normal and neurotic groups show no startling discrepancies, none of the five tests showing significant group differences.

The value and importance of carrying out a study with the additional control of the factors of 'set' and attitude is further indicated by a consideration and evaluation of the test performances of clinic children in the clinic situation. If the scores of this group are compared with the scores of the first group of clinic children tested away from the hospital, the following facts come to light. First the verbal tests. Of the six available comparisons five indicate a more neurotic score for the clinic-tested group. If the six scores are standardized and combined with equal weights into a 'verbal test score', the difference between the scores of caravan- and clinic-tested children approaches significance on a one-tailed '*t*' test ($t=1.650$), the clinic-tested group scoring more neurotically than the caravan-tested group. One must conclude, therefore, that testing individually in the clinic tends, on the whole, to evoke more neurotic responses to tests of this type. Of the performance tests eight similar comparisons are available, and of these all but one show a higher neurotic score associated with clinic testing. A combined 'performance-test score' derived in a similar way to the combined 'verbal-test score' shows a difference between the two groups of clinic-tested and caravan-tested children significant at beyond the 5% level ($t=2.282$) in the direction of more neurotic responses being associated with clinic testing. The conclusion would appear to be that the testing situation can and does affect the responses of neurotic children in such a way as to make them appear more neurotic than a similar group tested outside the hospital situation. Not only are the questionnaire responses affected in this manner but also the responses to the objective performance tests. This conclusion points to the need, in future work, for controlling the effects of the testing situation, or at least in some way allowing for them.

It is appropriate at this stage to draw attention to some additional discrepancies between the results of the present study and the results of earlier work. In particular, Himmelweit & Petrie found that the tests which best differentiated their normal and neurotic groups were the tests of static ataxia and suggestibility (a finding also in line with the results reported by Eysenck & Prell). On the other hand, the two tests of leg and dynamometer persistence were inefficient in this respect. In the present study the two tests of persistence are the most efficient, and the tests of static ataxia and suggestibility the least efficient in discriminating between the two groups, even when the clinic-tested group is used for comparison with the normal group in order that the two studies should be comparable. As long as different workers using similar tests on similar groups of patients continue to find such divergent results as these, little faith can be attached to any positive statement about personality dimensions other than that we are, so to speak, scratching at the surface. It has been shown above how the gross differentiation of normal and neurotic groups may be, to some extent, exaggerated by a failure to take into account the testing situation. A further difficulty at present looming large (Eysenck, 1952, p. 153) is consequent upon the disturbing possibility of non-linear regressions obtaining between test scores on the dimension of neuroticism.

Consider, for instance, the following diagram, bearing in mind the discrepancy between Himmelweit's results and the present results noted above.

Let the regressions of the four measures in question on the dimension of neuroticism be as shown. Let us suppose, further, that the normal and neurotic groups of the present study fall at points 'T', and that the groups in the Himmelweit study fall at points 'H'. From the diagram it will be seen that in the present study the persistence tests will discriminate more highly between the groups than will static ataxia and suggestibility; also that in the Himmelweit study suggestibility and static ataxia will discriminate more highly than leg and dynamometer persistence. If the regression of these tests is there fore as shown, and if the four groups of normals and neurotics do lie in the positions indicated, then the results of the two studies are consistent with one another. Two deductions can be made from the diagram. First, that for the two tests of persistence the variance of test scores should be *lower* in neurotic groups than in normal, and that for

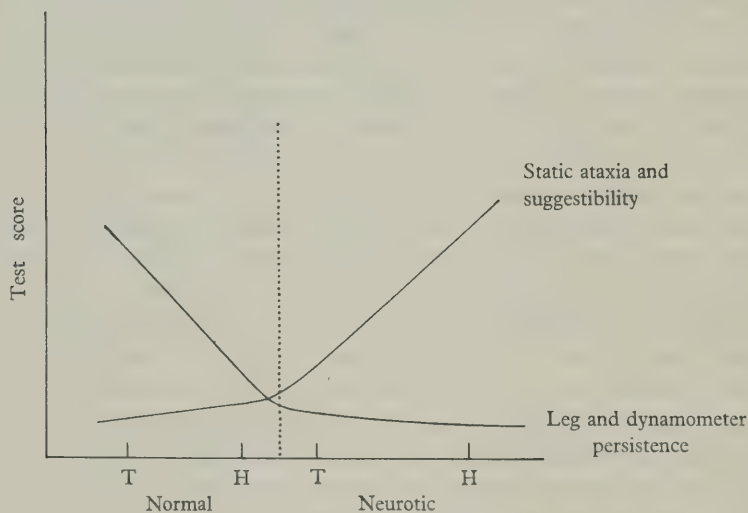


Fig. 1

static ataxia and suggestibility the variance of the scores in the neurotic groups should be *higher* than the corresponding variances in the normal groups. Secondly, if the means for the four groups on each of the four tests are compared, they should lie in the order depicted in the diagram. Regarding the first deduction, a glance at the table will indicate that the deduction is in fact borne out, the neurotics having higher test-score variance on static ataxia and suggestibility than the normals, and lower variance on the two tests of persistence. Regarding the second deduction, there is unfortunately a major difficulty, Himmelweit having used in her study a different dynamometer, and different static ataxia apparatus, and a different test of suggestibility. The test of leg persistence should, however, be comparable for the two studies, the only requirement being that the subject should be seated, and hold out his leg for as long as he possibly can. From the above diagram the group means should be arranged in the following order of magnitude, highest first: present-study normals, Himmelweit's normals, present-study neurotics, Himmelweit's neurotics. In fact the means are as follows:

Present-study normals	41.586	Present-study neurotics	29.793
Himmelweit's normals	34.021	Himmelweit's neurotics	28.024

These figures are in striking agreement with the deduction. It may be concluded, then, that the available evidence suggests that the discrepancies between the results of Himmelweit's study and the present one tend to disappear when account is taken of the types of regression probably obtaining between test scores and the underlying dimension, though more exacting evidence regarding these regression lines is at this stage desirable.

V. CONCLUSIONS

The results of this study point unequivocally to the need for adequate control over such variables as the testing situation and its associated attitudes whenever groups of normal and abnormal subjects are to be compared. It would indeed be of interest and importance to carry out a study with the present battery of tests along the lines of that of Lord (1950), whose work with the Rorschach test strongly suggested the dangers of indiscriminately administering that test. Not only have such controls up to now been omitted from studies concerned with neuroticism in children, but also from the more fundamental studies with adults, upon which the present investigation was primarily based. A recent report by Merson (1955), who demonstrated similar difficulties with the personality tests, would appear to support this contention. Few psychologists would be surprised at such a pronouncement on tests of the questionnaire type. The main import of the present experiment is that even tests of the objective performance variety are to some extent affected in a similar way, though probably to a lesser degree.

The results indicate quite clearly that when the influence of the testing situation upon test scores is taken into account, the questionnaire type of test has no value whatsoever in the discrimination of normal and neurotic children, though this sobering conclusion does not apply to the objective performance tests, most of them showing the sort of group differences that we have come to expect. It should be noted, however, that the obtained differences were smaller than has previously been the case.

A further pitfall in which research workers may indeed find themselves becomes obvious when it is realized that the customary way of identifying factors resulting from factorial analysis is to rotate them into maximum conformity with the discriminatory powers of each test (Eysenck, 1950*b*). As some tests are likely to be more affected by the testing situation than others, it is obvious that the final rotation will, to some extent, be determined by these extraneous factors. It is proposed to follow the present article with a second one which will deal with the factorial analysis of these new data unaffected by these extraneous variables.

There is some evidence in the above report, suggestive though not decisive, that the relation of test scores to neuroticism may be non-linear in some cases, and it has been shown how this may result in apparently startling discrepancies between the results of studies in which even the *same* tests have been applied to similar groups. A test showing good discrimination between two groups in one study may not discriminate at all between two similar groups in another if the only difference between the two studies is that all groups are selected from different points along the neuroticism continuum.

Finally, attention may be drawn to the central difficulty of all research based upon criterion group differentiation. Among adults it has been found that taking a wide range of emotional stability and instability into account, the reliability of psychiatric assessment of degree of instability was no better than 0.5 (Eysenck, 1952); in other words, there

is very little agreement among psychiatrists on the nature of the criterion. With children one might reasonably expect the reliability to be considerably lower, the bulk of evidence on the child's difficulties coming from the child's parents or teachers rather than from the child himself. The degree of normality of the normal groups of children is also often open to suspicion for the same reasons. One of the normal children of the present study (now 18 years of age) has recently been admitted to this hospital with a diagnosis of temporal lobe epilepsy with its associated behaviour disturbance. It is hard to see how this difficulty of criterion group selection may be circumvented, but it is clearly a central problem to this type of research.

The writers would like to express their appreciation to Prof. H. J. Eysenck who directed the present study; to the L.C.C. for their kind permission to test the children in their schools; to Drs Himmelweit and Petrie who gave generously of their time to help in the construction and selection of tests, and to the Directors of the Central Research Fund of the Bethlem Royal and Maudsley Hospitals, without whose financial help the study would not have been possible.

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COGNITIVE INFERENCE IN PERCEPTUAL ACTIVITY

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Whenever the sensory data are scanty or ambiguous, or incongruities occur in the perceptual situation, observers tend to employ processes of inferential thinking to arrive at satisfactory identifications. Such inferential thinking also appears when the observer is called upon to make accurate judgements about events which ordinarily might not be closely observed. In making inferences, observers utilize schematically organized knowledge and tendencies to react which they have acquired, either through experiences which are common to everyone as they grow up, or as the result of particular individual experiences and training. Thus some inferences and judgements are closely similar in different individuals, while others show marked individual differences. Evidence supporting this thesis is given in examples drawn from everyday life and from experimental investigations.

I. INTRODUCTION

seems a matter of some interest and importance to consider a type of cognitive process which is not often studied by psychologists, perhaps because it is hard to define and limit at all clearly; that is to say, the process of cognitive inference which follows immediate perception. This inferential thinking is probably different in nature from the more complex and highly developed forms of thinking—reasoning, abstract generalization and the like. Brunswik (1950) alluded to the processes which occur in identification through making inferences as ‘the implicit logic of perceptual quasi-inference’. These processes differ from the more complex types of thinking in that they are partial, intuitive and even non-veridical. What does occur is a rapid ‘hit-or-miss’ classification of the immediate percept into some previously established category of experience, or ‘schema’. At the moment we know little about the nature or structure of such schemata, nor how nor why any particular schema is utilized for a particular identification. All we can say is that, more often than not, identification is reasonably efficient and successful for all practical purposes.

To illustrate how schematic classification takes place, some examples are given here, together with a description of what seems to occur when it is utilized for identification. These examples demonstrate that the making of inferences is by no means always either accurate or logical. It works well enough in a rough and ready way; but it may lead to mistaken conclusions, which are ignored or overlooked when they are of minor importance from the practical point of view. If they are of major importance, then further perceptual investigation of the situation takes place, and is normally supplemented by more accurate and exhaustive processes of abstraction, generalization and logical reasoning.

II. THE DISTINCTION BETWEEN IDENTIFICATION AND EXACT OBSERVATION

It would probably be generally accepted that identifications are determined, not only by immediate sensory data, and by the expectancies set up in an experimental situation, but also by a more general type of cognitive influence—that constituted by an all-round knowledge of the kinds of experience which occur in particular kinds of situation, and of the kind of behavioural reaction which is most appropriate to them. But there are some situations, and in particular those encountered in laboratory experiments, in which it

might be expected that an observer would be completely at a loss, because he would never have encountered just such a situation before. It is true that he is sometimes confused and uncertain, and even that his confusion may persist. But more often he arrives at some identification of the data presented to him which satisfies him. Sometimes these identifications may be very different for different observers. In other cases they may be similar; or may fall into two or more types of similarity. But in all cases they seem to indicate that the observer has made some inference from his sensory impressions in order to 'make sense' of the situation; he thinks he knows what is there, and how he intends to react to it. Thus it may be said that the observer has apprehended the whole situation to his satisfaction. (The notion of 'satisfaction', of knowing how to handle the situation, appears to be more in accordance with the concept of 'functional validity' (see Brunswik, 1955) than that of 'correctness'—that is to say, of correspondence of the perceived with the stimulus object. It is true that the latter may ultimately produce a more successful adjustment to the environment; but it cannot be assumed that it is the natural and invariable goal of the observer's activities.) What he has identified agrees with his previous knowledge and experience of that kind of situation, and he knows how to handle it. But this does not mean necessarily that his identification is veridical. Ordinarily of course he will have little difficulty in using the incoming sensory data to identify particular objects. But sometimes what happens to the observer is less simple; though the complexity may be overlooked if insufficient note is taken of the exact nature of the experience of a single observer, or of the different experiences of different observers. If the sensory data are vague, ambiguous or complex, one observer may say: 'It looks like an X', or, 'It might be an X'. Another observer may say: 'It might be a Y', or, 'It looks like an X, but I think it is probably a Y'—the second part of this statement being based upon his expectations, or upon certain other corroborative data, perhaps supplied by sensory data of another mode. Of course in these situations we are never quite certain that different observers are stimulated by the same sensory pattern. But it is unlikely that this pattern varies to the extent that the reports and descriptions of different observers vary. It seems far more probable that the different observers attach a different significance to the various details of the sensory pattern, and therefore identify it differently. Moreover, the same observer may at one time attach more importance to what he thinks the object *is*; and at another time, to what, momentarily, it *looks like*. Not infrequently an observer will say: 'It looks like an A, but I *think* nevertheless that it is a B'.

This is the kind of difference which occurs, for instance, between the so-called 'sensory' and 'object' judgements of the constancy experiments, which may seem to exist simultaneously when at one and the same moment we perceive things as having their 'real' sizes, and also fitting into the particular angular segment they subtend at the eye. In the 'object' judgement, the one we most commonly use in everyday life, we are concerned mainly with what the object *is*, rather than what it looks like. Thus we wish to know whether it is a shadowed surface, or one with parts in relief; whether it is a blue fabric or a greenish one; whether it is a man standing on the mountainside, or a rock twenty feet high but a long way off. These are instances in which judgement is difficult to make and often at fault;* though there are, of course, innumerable situations in which we make the

* An instance of this occurred recently to the writer and a friend. An object moving along the sky-line of a hill was identified by the writer as a rabbit, and by the friend as a bird. It proved to be the upper part of a man's body, the lower part of which was hidden by the hillside.

identification easily and correctly. The 'sensory judgement' is less frequently utilized in everyday life, except when the sensory qualities of the stimulus are markedly in conflict with what we infer to be its identity. For instance, we might notice specifically the brilliant bluish purple of the mountains of Skye seen from a distance; or a sea which looks like molten copper in the rays of the setting sun. These aspects are striking and interesting in themselves, more so than the nature of the particular object to which they appertain. Such percepts are the raw material of artists and poets. The biologist also is trained to perceive the exact appearance of the specimens he examines, rather than to make an immediate and possibly erroneous identification (but cf. p. 43 below).

There are occasions, even in everyday life, when we are misled by such unusual sensory impressions to make an incorrect identification because our knowledge and our expectations are inadequate to confirm or contradict them, at least momentarily so. Starting at a shadow seen peripherally may happen easily. Two other examples drawn from the author's experience are: stumbling on a stair which was thought to be only a shadow; and the impression that the railway cutting was sliding down on to the line behind the train, although what was seen was only the induced after effect of relative movement. These mistakes are probably frequent enough, and most of us make them from time to time. Adams (1923) has described a number of similar instances. But usually we ignore or forget them, because they are not corroborated by other aspects of the situation; or because we proceed to check our assumptions by some form of action which shows us that they were non-veridical. But momentarily we identified them in that way, though clearly the sensory data could not in themselves have given rise to that particular identification.

III. AMBIGUOUS IDENTIFICATIONS IN EXPERIMENTS WITH REDUCED SENSORY DATA

Recently a number of similar instances have been demonstrated under experimental conditions of the anomalies in identification of objects which may occur in certain special circumstances, particularly when the sensory data are scanty or ambiguous.

There is a number of demonstrations and experiments by Ames, Hastorff and Ittelson in which the apparent shape, size and distance of objects, seen monocularly with surroundings obscured, appeared to be determined by what the observer believed to be the identity of the objects. Thus Ames (1951) stated that a trapezoidal-shaped window frame rotated about a vertical axis appeared as a rectangular window frame oscillating to and fro. Hastorff (1950) required subjects to adjust the size of a stimulus object, seen monocularly in a dark field, till it appeared equidistant with a standard object seen binocularly. The setting of apparent equidistance was farther away when the variable was given the name of a comparatively large object (envelope, billiard ball) than when named as a smaller object (calling card, ping-pong ball). Indeed, the size of the variable was adjusted until it equated that of the named object seen at the distance of the standard. Ittelson (1951) found that the distance of known objects such as a playing card and a match-box could be judged accurately when they were seen monocularly with surroundings darkened; and half-sized and double-sized playing cards were judged to be double or half their actual distance. When two ink blots were presented, one double the size of the other, the former was usually judged to be about half as far away as the latter. But this judgement was more variable than the judgements of position of familiar objects.

Now the explanation given for these phenomena has sometimes been that what is

perceived is determined by the subjects' familiarity with particular objects, rectangular windows, playing cards, and so on. Hochberg & Hochberg (1952) have controverted this hypothesis, and advanced in evidence for their contentions an experiment in which they presented to monocular vision two screens, one having on it the drawing of a boy, the other of a man of exactly the same size. It might be thought that the subject would suppose the latter to be farther away than the former, because he would expect a drawing of a man to be actually larger than that of a boy. In fact, there was no consistent tendency for the man to be located as farther away than the boy. But if two pictures were shown of the same boy, one large and the other small, the latter was judged to be farther away than the former. Thus it seemed that the essential factor was the identification of the two percepts as having been produced by identical stimulus objects of the same size, placed at different distances.

Again, Gogel, Hartman & Harker (1956) presented double- and normal-sized playing cards at different distances, with monocular vision and an otherwise dark field, and compared the estimates of distance for each of the conditions when it was the *first* estimate made by a particular group of subjects. In other words, the subjects were making absolute estimates of distance, unrelated to other estimates made in similar circumstances. The relationship demonstrated by Ittelson did not hold. There was no tendency to see the double-sized card nearer than the normal-sized card. But in subsequent *comparative* judgements, the perceived distance did vary inversely with the retinal size. Thus familiarity with the particular object was not sufficient to define the situation.

In the Ames experiment, it was postulated that the rotating trapezoidal window was perceived as an oscillating rectangular window because one is accustomed to seeing rectangular and not trapezoidal windows. This does not explain why a cross-bar attached to the frame was seen to bend at various points during the movement. Although this may have been necessary to preserve the impression of rectilinearity of the window, it is certainly not in itself a customary experience. Again, Mulholland (1956) has recently shown that the perceived movement of the trapezoidal window may vary quite considerably in character, especially at high speeds of rotation. Sometimes it may appear to rotate, whereas a rectangular window may seem to oscillate; or both may expand and contract.

Now Ittelson, Kilpatrick and Cantril (see, for instance, Kilpatrick (1955) and Toch & Ittelson (1956)), the protagonists of the 'transactionist' approach, consider that perception in such ambiguous situations is a function, not so much of the assumptions made as to the nature of particular objects, but rather of the assumptions based upon a 'weighted average' of previous experiences and of the present stimulus conditions, so that the response is the 'best bet' in the particular circumstances. There are certain situations in which the previous experience of different observers is so similar that they make 'bets' which are approximately the same. But it is clear from the experiments quoted above that different observers often made different 'bets', and that these varied in different conditions in unpredictable ways. This may have been the result of differences in previous experience, or of different weightings of that experience. What seems most probable is that different observers in different circumstances made different inferences as to the nature and conditions of the situation. Thus it is not possible to predict beforehand, without experimental investigation, just what inferences will be made, and what previous experiences will be utilized.

IV. THE FUNCTIONS OF CONFIGURATIONAL CLASSIFICATION

rather a different light is thrown upon these conclusions by experiments in which the perceived configuration, although its formal characteristics appear quite definite, nevertheless differs radically from the stimulus data. After the configuration has been perceived, some further cognitive process supervenes, and the observer describes or defines the percept in terms of some familiar object or experience, which commonly differs for different observers. Here there appears to be an original perception in terms of some recognizable configuration with characteristic formal properties; further inferences are then made in order to produce a meaningful identification, but it is doubtful whether they greatly influence the original percept.

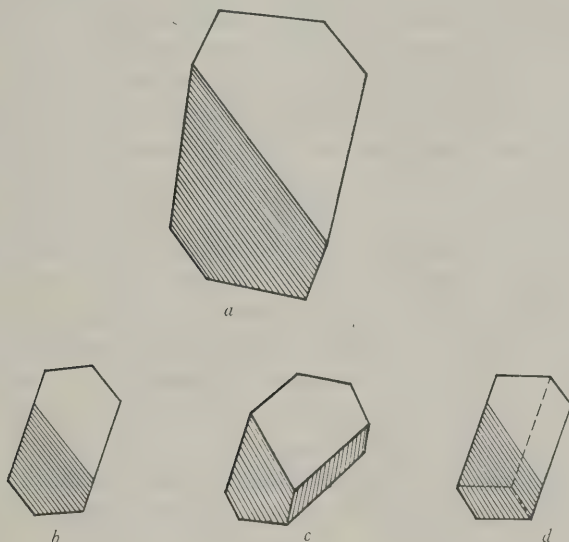


Fig. 1

The first example of these phenomena is given in two sets of experiments by Johansen (1955). In the first, he presented a complex solid made up of two interlacing cubes, in three different spatial positions. The subjects did not know that they were seeing the same object in each of these positions. In the first position they reported seeing a flat shaded surface at right angles to the line of sight; in the second, an object which they called 'mussel' or a 'lampshade'; and in the third, the two interlacing cubes. In general, they thought they were seeing a new object in each of these positions. It was only after repeated presentation of the three positions in succession that they reasoned out for themselves that it was one and the same object.

In another experiment, observers were shown an irregularly shaped solid (see Fig. 1a) at different distances, in binocular vision with daylight illumination. When far distant (about 6 metres), it was seen sometimes as a flat surface, parts of which were shadowed (Fig. 1b); and sometimes as a solid object which they likened either to a 'coffin' or the model of a barn', or to a 'beam', part of which was shadowed (Fig. 1c, d). When quite close, it was seen as quite a different object, which was stable but too complex to be

named; it could be described only in abstract terms. Furthermore, when they were asked to estimate, by matching, the distance of the object in its first position, their estimates varied according as to whether it was seen as a flat surface, a 'coffin' or a 'beam'.

Hence it appears that the subjects in these experiments first of all perceived the shape characteristics of these objects. They could not identify them exactly in terms of any familiar object—they merely said that they resembled familiar objects. But each perceived configuration was sufficiently definite for them to differentiate it from the other configurations, and to localize it in space. There was no gradual change between one configuration and the next; the point of transition was quite sharp.

Rather similar processes occurred in the movement phenomena described by Michotte (1946) and Johansson (1950). Michotte first demonstrated configurations which consistently had the appearance of mechanical causation, in which the movement of an object was generated by the impact of another moving object. He then presented other configurations which had the appearance of autonomous self-caused movement, in which the expansion and contraction of an object was perceived together with simultaneous translatory movement. This distinction was paralleled in the experiments of Johansson (1950). He presented two black spots moving simultaneously in different ways upon a translucent screen. He found that certain movement configurations were perceived which differed radically from the real movements of the two spots. It was as if in the perception of the movements made they were analysed and re-combined, resulting in the appearance of the new configurations. For instance, two spots moving in opposite directions round circular tracks which touched at one point, were seen as two spots approaching each other along a horizontal line, fusing and separating again; and at the same time the path of their motion rose and fell vertically. Johansson investigated a number of these phenomena, and found that in certain cases, when the movements were referred to a common centre which was at rest, they usually produced the effect of some kind of rather rigid mechanical movement, and were so described by the observers. But in more complex types of configuration, where movement was perceived in relation to a moving centre or frame of reference, it was associated with the more flexible and complex movements of living creatures.

The impression produced by these observations is that quite definite and unambiguous perceptual configurations were perceived, which could not be attributed to the characteristics of the stimuli. They varied to some extent in different observers, and so also did the meaningful identifications given them; but the variations were limited in number. Neither could the perception be said to be a function of identification in terms of some familiar object or meaningful experience. But at the same time the observers spontaneously classified and described their perceptions in terms of such experiences. We may perhaps conclude that, in a non-laboratory situation, an observer would seldom be content with perception of the configuration as such, but would be almost certain to proceed to some process of inference, explanation and classification in terms of familiar experiences, in order to suggest the most appropriate type of behavioural reaction. Nevertheless, the experiments demonstrate the existence of primary schematic classifications in terms of the particular formal characteristics of the configurations.

V. DIFFERENT TYPES OF REACTION IN CONSTANCY JUDGEMENTS

It is clear then that it is not unfamiliarity or inadequacy of the sensory data which in themselves lead to variable and ambiguous identifications. It is also possible to find instances of similar effects in conditions approximating to those of everyday life, for instance in judgements of size and distance associated with the size-constancy phenomenon. We have already noted that constancy judgements may vary according as the observer makes the 'sensory' or the 'object' judgement; that is to say, according to his expectations and intentions in judging the sizes of objects. But these types of judgement are not made with equal ease and spontaneity; and they may vary according to the particular situation in which they are made. Thus Gilinsky (1955) found that with objects of unknown size at great distances (up to 4000 ft.), observers experienced little difficulty in following instructions to make 'retinal' ('sensory') judgements of projected size, and made them fairly accurately. But 'objective' judgements of real size were uncertain and difficult to make, and sometimes involved a considerable amount of reasoning. Joynson (unpublished work) found that with comparatively near objects, the usual spontaneous tendency was to make matches of real size, which were reasonably accurate, by 'allowing for differences of distance'. The observers could also match with fair accuracy according to projected size, when required to do so, by judging the angles subtended by the objects. But unless this method of matching is employed, judgement of projected size is rarely very accurate; according to Piaget & Lambercier (1951) it is more accurate at the age of 7-8 years than in older children and adults. With familiar objects at near distances, the normal method appears to be to make judgements of real size by utilizing the known properties of the objects. Bolles & Bailey (1956) have shown that such judgements can in many cases be made just as accurately without looking at the objects at all.

Thus, in different situations, and with different types of intention on the part of the observer, the size which is immediately perceived may vary. Still other types of appearance may occur in other everyday life situations, for instance when the observer makes a rapid snap judgement about an object of known size, such as a man or a car, at a great distance from him. He may say: 'I think that must be a man although he looks so tiny at that distance.' Or if it is an unknown object, he may say: 'That looks a smallish object, but I know it is a great way off, so it may be a good deal bigger.' Or if he is looking at a photograph of distant mountains, he may say: 'Those don't look very high, but when I saw them myself, they looked much higher.' In other words, apparent size seems to oscillate in some way between values approaching those of real and of projected size. Or both percepts may co-exist, constituting what Piaget (see Michotte, Piaget & Piéron, 1955) has called a 'phenomenal doubling', the one being a quality of what the object is thought to be, the other of what it looks like. However, when the observer is asked to make an accurate quantitative estimate, then he uses his knowledge of the situation to arrive at inferential judgments which tend to fall at one or other of the two polar positions according to his intentions and purpose in making his estimate. These intentions are directed to a greater or less extent by the experimental instructions, but it is impossible for the experimenter to ascertain to what extent. Indeed, it is unlikely that the observer himself can either direct his judgements voluntarily to more than a limited degree, or can be certain how far he has done so. Hence the result of almost all experiments on constancy, that individuals differ from one another in their judgements; and

indeed, may be inconsistent with themselves on different occasions, because they have been making different inferences. Whether the actual percepts vary in the same way is impossible to ascertain; but what the observer does with these percepts undoubtedly does vary.

VI. AMBIGUITIES IN THE IDENTIFICATION OF PICTORIAL MATERIAL

A rather different type of inference made from an original percept was studied in an experiment by the author in which the subjects were shown tachistoscopically a series of twelve photographs of scenes and objects of an unusual character, or photographed from an unusual viewpoint; and a series of twelve reproductions of modern paintings. Almost all of these were difficult to identify. From each of the series, three examples were selected which the subject had failed to identify, or had identified incorrectly. These were presented a week later tachistoscopically, together with nine new photographs and nine new pictures. No indication was given the subjects that they would be shown any of the same material again. But it was found that 74 % of the repeated photographs and 64 % of the repeated pictures were recognized immediately and spontaneously as having been seen before. It was not a question of mere general familiarity, but of specific recognition. There were other cases in which the repeated photographs or pictures were reported as being vaguely familiar, or similar to ones seen before; but the experience was obviously a different one.

However, in many of the cases of true recognition, it was the patterns of light and shade and colour that were recognized, and *not* the identification which the subject had made. In other cases, the subject said: 'I remember that, I called it A'; often adding, 'But now I can see it is B', or, 'I can see B as well'. Thus it seemed fairly clear that, when material was difficult to identify with any certainty, the subject often perceived a meaningless visual pattern with sufficient clarity to remember it and discriminate it from other similar material.

But it appeared that the difficulty in identification was not due to the inadequacy of the sensory data. Admittedly these were brief in appearance; but the exposure of $\frac{1}{2}$ sec. was long enough for perceiving simple and unambiguous stimulus material (as was obvious with one or two simpler photographs and pictures). Moreover, the majority of examples were not too vague or dim for discrimination; hence the possibility of subsequent recognition. The difficulty appeared to be either that the objects presented were themselves unfamiliar or were shown in unfamiliar aspects; or that the objects were so incongruous with each other that no consistent hypothesis could explain or reconcile them. It is easy to see how this could occur with examples of modern abstract art. For instance, a composition by Picasso contained a nude torso, a bust and a pattern of leaves, all strongly outlined in black. The latter characteristic gave rise to the frequent response that it was a stained glass window. The nude, though extremely obvious, was omitted by seven of the nine subjects. Other pictures were reported as 'incongruous' or 'not making sense'.

It seems fairly clear then that with this material the subjects were striving to make some meaningful identification of what they perceived, and that inferences were difficult to make because they did not 'fit' one another, or what the subject expected to see,*

* In this case, what the observer would expect to see or would regard as fitting would be pictures which are 'realistic' in accordance with the convention of realism in art which is normally adopted today. As Arnheim (1956) has pointed out, the convention of realism varies from age to age, and present-day 'realistic' pictures are in fact no more exactly representational of the objects they depict than are, for instance, Egyptian sculptures or medieval paintings.

though some assistance was obtained from the judgement that the pictures were examples of modern abstract painting. Now it is perfectly true that such instances may not occur very frequently in ordinary everyday life. But that they are of considerable importance in situations existing outside the psychological laboratory is demonstrated by recent discussion in the *Lancet* on the difficulties and disagreements between experts in medical diagnosis, and in particular in the reading of X-ray photographs. Johnson (1955) has discussed the differences of diagnosis of chest radiographs made by the same radiographers with the same radiographs after 2 months' interval; and also by American and British radiographers examining the same radiographs. These latter differences were apparently due to the fact that, because the American experts had had much experience of diagnosing pulmonary lesions from very faint shadows, they noticed these more readily than did the British experts. The latter, however, with their extensive experience of pneumoconiosis in coal-miners, more quickly noticed the signs of the changes produced by this disease.

Now Johnson (1953) had previously shown how medical students tended in making drawings of specimens or reports on X-ray photographs to confuse what was really there with what they thought ought to be there. Indeed, some of the students who could not identify a specimen made a better drawing of it than some who did identify it, since the latter tended to make their drawings like the text-book diagrams. It was possible to train the students by discussion and observation to note what was really there rather than what they thought ought to be there. But clearly both the students and the experts were liable to fit their observations to their inferences, and to ignore material which did not fit. In the course of training, the students learnt to be more accurate and objective, to observe appearances rather than to make quick identifications. But the experts had developed new sets of hypotheses (though no doubt of a superior kind) which once more determined their inferences and directed their observations. This controverts to some extent the statements of Gibson & Gibson (1955), that learning automatically and necessarily produces more veridical perception and more accurate discrimination of sensory detail. No doubt the expert radiographers did observe more accurately than did the students; but their observations were still limited by inferences based upon certain required hypotheses.

VII. THE DEVELOPMENT OF INFERENTIAL THINKING

Some light is thrown upon the development of inferential thinking in one of its earlier stages by the observations of Piaget & Stettler-von Albertini (1954). They studied the actions of children from 3 to 4 years upwards when they were shown superimposed simple forms (circle, square, etc.), forms with parts of their outlines distorted or incomplete, and superimposed incomplete forms. It appeared that even quite young children (under 4 years) could perceive simple 'good' forms such as circles and squares directly and immediately. But if these were superimposed, still more if they were mutilated or incomplete, some form of 'perceptual activity' was necessary before the child could re-constitute the simple 'good' forms; that is to say, he had to analyse the stimulus material into its parts, compare these parts, image new parts, and co-ordinate them into a new 'whole'. This activity required at first much trial and error, but it resulted in the building up of perceptual schemata relating to geometrical forms which earlier were perceived

passively. The schemata could subsequently be utilized in the perception of these forms in many different settings, for instance, when the stimulus forms were superimposed or incomplete. When this had occurred, the child was then able without further perceptual activity to perceive the completed forms immediately. But before children were sufficiently mature to perform the perceptual activity necessary for completing the forms, they were liable to resort to earlier perceptual schemata, those constituted by certain classes of real objects. They were therefore apt at this stage to perceive incomplete and mutilated forms as representations of real objects.

Now Piaget & Stettler-von Albertini state that this type of perceptual activity develops before the child is able to use intelligent reasoning in making perceptual judgements. Piaget had pointed out earlier (1950) that in perceptual activity the observations made are still liable to syncretism and to ego-centricity, to distortion by reference to the framework and through the influence of particular 'sets'; whereas intelligent judgements are 'de-centred' from the self and from the particular perceptual framework. Nevertheless, these processes are not completely isolated from each other. As the child matures, perceptual activity is performed with a greater degree of intelligence; and the perceptual schemata built up are reinforced and modified by intelligent thinking. Thus as soon as the child ceases to accept the sensory data passively (which must occur much earlier with many real-life situations than with meaningless geometrical forms), he begins to make inferences as he actively directs his observations from one aspect of the environment to another, analysing and comparing what he perceives. At each stage, perceptual data are recorded and organized in schemata, which in turn direct subsequent perceptual activities, until the schemata are themselves modified or even discarded. Finally the developing intelligence will intervene whenever the inferences based upon the schemata are found unsatisfactory, and the individual will try to base his judgements on reason. In other situations, such as making constancy judgements, the child gradually learns to make use of corroborative data, such as are afforded by objects intermediate in position between the standard and the variable (see Lambercier, 1946). It is clear that perceptual activity is also involved here.

Some evidence on the functions of perceptual activity, as modified by the effects of an entirely new set of environmental conditions, may be derived from the experiments of Kohler (1953) on the wearing for prolonged periods of spectacles which reversed the visual field. It was found that voluntarily controlled movements could more readily be adjusted to the new conditions than could habitual movements such as walking and reaching for objects. But these both preceded the adjustment of conscious visual experience. In time, conflicting impressions were ignored, but for a while only partially—that is to say, some parts of the field showed reversal, others did not. Things such as unfamiliar printed words, which had to be considered attentively, still appeared in mirror-reversal, after familiar words had changed to a normal appearance. A critical attitude towards visual phenomena aroused old memories of the earlier appearances of objects. Thus it seems that the perceptual schemata were modified and reformed in piecemeal fashion; that they were most easily re-established in accordance with kinaesthetic data consciously experienced in voluntary non-habitual movement; and that the more complex and unfamiliar the visually perceived situation, the more probable it was that inferential thinking would be utilized to balance previous knowledge against immediate visual impressions.

VII. CONCLUSIONS

From these various observations and experimental results we may conclude that, whatever the observer makes of his original percepts (whether or not he is actually conscious of them), usually appears only in the final identifications he gives to them. These identifications may often depend upon processes of inferential thinking and judgement. We may not be aware that we have made such inferences because they have been rapidly and easily made, and easily checked and verified by overt action. Or we may dismiss them from mind when a satisfactory identification is attained, because they are no longer important. Even in unfamiliar and incongruous situations, we rapidly select the most sensible inferences, and ignore or reject the incongruities. If the latter are too glaring and we cannot ignore them, then we may embark upon a process of explicit reasoning in order to reconcile them. But the inferential process may appear more frequently in experimental situations than in ordinary life, because in the former the observer is called upon to make accurate judgements, often quantitatively accurate, of phenomena to which ordinarily he might pay little attention. But the fact that he so readily resorts to the process of inference argues that he may use it more than he realizes in everyday life. If immediate inference does not lead the observer to satisfactory conclusions, particularly if these should prove to be obviously non-veridical, he will endeavour by means of perceptual activity to examine the field with more care, perhaps utilizing other sensory data by handling and manipulating objects. It is only in the last instance, normally, that he is compelled to resort to intelligent reasoning in order to achieve a satisfactory identification (see, for instance, Johansen's experiments described on p. 39). Often social influences are necessary to bring home to him that his observations do not correspond with those of others. Thus Johnson (1955) has pointed out that disagreements in radiographic diagnoses may be resolved by discussion between radiographers.

Little is definitely known as to the nature and the course of the process of inferential thinking which occurs in making judgements on perceptual data. It is so rapid and facile as to be very difficult to observe introspectively; and, indeed, much of it may take place outside consciousness. Presumably it must employ ideas of fittingness, what events agree and belong together, what makes sense; and, as Piaget and Stettler-von Albertini have shown, these ideas develop gradually as we grow up. When the stimulus conditions give rise to ambiguous or incongruous impressions, when we do not know how to react appropriately, we match and compare these impressions with organized memory material, in order to find some explanation which fits existing phenomena to our ideas and tells us what to do. This memory material is organized along the lines indicated in Bartlett's theory of schemata (1932). But the types of schemata employed are obviously many and various. Sometimes they relate to rather narrowly limited classes of familiar objects, as in the playing card and match-box experiments of Ittelson (see p. 37). Sometimes they have been specially developed by training to cover much more complex perceptual situations, such as those described by Johnson (1953, 1955). But many of the experiments described in this paper have demonstrated the functioning of schemata covering broad categories of phenomena, such as those of geometric form, of related shapes and orientations, of related size and distance, of relative movement, and of spatial orientation. It is probable that within many of these are organized not only visual data, but also much tactile data related to impressions of voluminousness and solidity; and of proprioceptive

data, related to position and movement of the body in space. Thus the work of Witkin & Asch (1948), of Werner & Wapner (1952) and of Kohler (1953) has shown that the relation of bodily orientation to the perceived position of objects provides highly labile and complex perceptual data. When measurements of these positions are involved, the inferential judgements show a general schematic determination, but are nevertheless very variable between different individuals.

It must be emphasized that the schemata which are utilized in making inferential judgements in general include response tendencies as well as cognitive data. They give some indication as to the most appropriate form of behaviour in response to the cognized situation. Thus when an observer wishes to utilize his observations in some particular way—to make some particular type of judgement—he spontaneously resorts to the appropriate schema. This classifies and identifies his experience; it suggests hypotheses as to the nature of that experience; and it indicates the manner in which he should act, how he should handle the situation effectively. Furthermore, the consequences of action, and the further perceptual data resulting from action, in turn modify and adjust the schemata in order that they may be more adequate to control perception in future. The 'weighted average' is adjusted, and the best 'bet' altered appropriately.

It is the purpose of this paper to draw attention to the part played by inferential thinking in so many of the judgements that are made in perceptual experiments, and probably also in many of the experiences of everyday life. Whatever the nature of the immediate percept—and that is usually hard to determine—it is the inferential thinking which largely directs the activities involved in making these judgements. It is fashionable nowadays to draw attention to the influence of motivational factors and personality characteristics, particularly in producing non-veridical perceptions and individual differences in perception. But except in exceptional circumstances (for instance, of violent need, when the sensory data are minimal), no evidence has been advanced that the effects of these factors are other than slight. They may possibly accelerate or retard perception; or, as the experiments of Smock (1955) seem to indicate, they may cause an observer subjected to undue stress to jump too quickly to conclusions based on inferences made while sensory data are still inadequate. But the part played by schematic categories of experience (which are, of course, frequently affected by motivation), and the inferences to which they give rise, is more than influential; it is determinative. Organized knowledge of the kind of events that occur in certain situations, and the kind of reactions which are appropriate, permeates and directs our whole present experience. Personality factors, in so far as they are of importance, may stimulate us to acquire certain kinds of knowledge, upon which are based our hypotheses about the situation, and hence the aspects of it which we perceive.

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PERSONALITY AND PERCEPTION: THE CONSTANCY EFFECT AND INTROVERSION

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An investigation of the relationship between shape constancy and introversion-extraversion was carried out, using as subjects twenty-four first-year Psychology students of Aberdeen University; twelve introverts and twelve extraverts, selected on the basis of the Minnesota TSE scale. Results showed (a) that introverts showed lower constancy effect than extraverts and (b) that there was a tendency for men to show greater constancy effect than women.

There was also some suggestion of the presence of a learning factor in the perceptual process which also distinguished introverts from extraverts. These results are discussed in the light of previous investigations. Hypotheses, which are shortly to be tested, and which might account for the findings, are advanced.

I. INTRODUCTION: SETTING OF THE EXPERIMENT

In the course of experiments with mescaline, it was noticed that the incidence and intensity of perceptual abnormalities varied with alterations in attention to the environment. On the one hand, so long as a practical, reality-orientated, extraverted attitude was maintained (often with difficulty), the environment would be made to appear approximately normal. On the other hand, when subjects lapsed into a dreamy preoccupied state, they experienced marked perceptual distortions. These included hallucinatory movement, tunnel vision, the appearance of networks superimposed on real objects, and various other distortions which have been reported by many other workers with mescaline. The subject has recently been reviewed by Mayer-Gross (1951).

In our own experience, a regular feature of the distorted perceptions under mescaline was a reduction of the constancy effect—one important aspect of a more general disorganization of perception. For example, one subject, viewing a door somewhat obliquely remarked that it seemed to be shaped like a wedge and that it required a definite effort to see it as he knew it to be, i.e., rectangular. This particular aspect of the disorganization reduced constancy, or in Thouless's terminology reduced 'phenomenal regression to the real object', corresponded so closely to the presence of an introverted attitude that it was decided to investigate further the relationship, if any, existing between the constancy effect and the personality trait of introversion in normal individuals.

The role of personality factors in the perceptual process has received considerable attention in recent years. The work of Brunswik, Bruner and Postman, Frenkel-Brunswik, Klein and Schlesinger, and many others in this field, requires no exposition in this article. The area with which we are particularly concerned, the constancy phenomenon, has itself been the subject of a considerable literature. The factors affecting constancy (e.g. the number of cues present, the attitude of the observer, the role of learning, and the developmental aspect of the phenomenon) have all been investigated, and experiments on them have given rise to a number of controversial issues. The question of attitude

which is particularly relevant to our own research, has been studied by Klimpfinger (1933*a*) and Holaday (1933) working under Brunswik. They demonstrated that the adoption of an analytical attitude on the part of the observer results in a marked reduction of both shape and size constancy.

Relatively few researches, however, have been carried out on the more specific question of how persons, who are known to differ on a given personality dimension, behave in a constancy experiment. Thouless (1932) did carry out one such investigation. In the course of previous experiments, investigating individual differences in 'phenomenal regression', he had observed that 'those of introverted or schizothymic temperaments saw much more closely to the stimulus or perspective object* than did the extraverted or cyclothymic. It seemed even possible that this might be a measurable result of the greater interest in outside reality of the cyclothyme and in the subjective world of the schizothyme' (Thouless, 1932). An experiment in which fourteen cyclothymes and fourteen schizothymes (selected on the basis of Kretschmer's descriptions) were tested, gave results which, though they failed to reach significance, suggested nevertheless that schizothymes did experience phenomenal regression less markedly than cyclothymes.

In a later study, Weber (1939), using stereoscopic material, confirmed that extraverts showed greater size constancy than introverts, but reported non-significant results for shape constancy, although the tendency was in the expected direction.

In our experiment we are concerned with shape constancy, and an attempt has been made to control experimentally, or partial out statistically, factors which are known, or thought, to influence the constancy effect. It was predicted that extraverts would show a more marked constancy effect in their perception than introverts.

II. CRITERIA OF INTROVERSION-EXTRAVERSION

For the purposes of this experiment, introverts are regarded as those individuals who have a low score, and extraverts those with a high score on the Thinking dimension of the Minnesota TSE scale of introversion-extraversion devised and standardized by Evans & McConnell (1941). The authors constructed this scale to measure three types of introversion-extraversion, Thinking, Social and Emotional, which were isolated by Guilford in his factor analysis. This analysis yielded evidence that the available I-E tests were not measuring a single dimension of personality, and Evans and McConnell devised three distinct sets of item in an effort to develop relatively independent tests dealing with thinking, social and emotional reactions. The result is a scale providing three measures of I-E, which have been shown by the authors to be almost uncorrelated. It was on the first of these, the Thinking I-E score, that our subjects were selected.

III. SUBJECTS

The subjects were drawn from the first-year class in Psychology in Aberdeen University, and their ages ranged from 17 to 24 years. Out of a group of ninety-one previously rated on the TSE Minnesota Scale, twenty-four students were selected: these comprised the six highest and six lowest scorers on the Thinking dimension among the men, and the six highest and six lowest scorers among the women.

* By this Thouless means the object as it appeared optically or in contradistinction to its true nature in objective reality.

IV. APPARATUS AND PROCEDURE

Three stimulus objects were employed; a disk 10 in. in diameter, a square of 10 in. diagonal, and a rod 10 in. long and $\frac{1}{8}$ in. thick. All were of metal and painted white. They were supported by means of a clamp and stand and a rotating horizontal metal bar. They were presented to the subject either vertically—when the entire surface (or in the case of the rod the entire length) was visible, or horizontally, when only the edge of the circle or square (or the end of the rod) was visible, or in one of several intervening positions. The actual angle of inclination to the vertical was measured by a suitably attached protractor.

The subject was provided with a chin rest, adjusted to a comfortable height, and the height of the first stimulus object, the metal disk, was adjusted to eye level so that, when it was in a horizontal position, it was seen by the subject as a straight line and, when vertical, as a complete circle. The subject was also provided with ten white cardboard ellipses, all with major axis 10 in. long and with minor axes ranging from 1 to 10 in., and numbered accordingly.

Instructions were standard for all subjects. It was pointed out that the circle, when tilted, would appear to the subject to be elliptical; he was to choose from the ten ellipses given to him, the one which looked nearest in shape to the tilted circle, and call out its number. He was allowed to give intermediate values when the tilted circle seemed to him to fall between two of his ellipses. When inspecting the cardboard ellipses, he held these at right angles to his line of regard and at arm's length, the stimulus object being 7 ft. away, against a black background.

A similar procedure was followed with the square and the rod, the square being presented with its diagonals placed vertically and horizontally. In the case of the rod (which, in the horizontal position, was end on to the subject), he was provided with ten rods ranging from 1 to 10 in. in length from which he had to choose, and for the square a series of diamond-shaped pieces of cardboard, one diagonal always being 10 in. and the other 1 to 10 in.

V. STATISTICAL TREATMENT OF RESULTS

The two estimates obtained for each presentation of each stimulus object were averaged for each subject and the final results, seven readings for each of three kinds of stimulus object for each of twenty-four subjects (a total of 504 readings), were analysed. Analysis of variance was used, with four main variables: I-E, stimulus object, sex, and angle. Results of this analysis are given in Table 1.

Table 1

Source	<i>F</i> -ratio	<i>P</i>
Introversion-Extraversion	9.71	0.001
Angle	198.70	0.001
Stimulus	<1	—
Sex	2.96	0.10
Sex $\times$ stimulus	1.99	—
I-E $\times$ stimulus	1.50	—

The main variable, I-E, gave highly significant results, with an *F*-ratio of 9.71 which with d.f. 1 and 420 gives $P < 0.001$. As predicted, introverts showed markedly less 'phenomenal regression' than extraverts, and this was true for both sexes, and for a

three stimulus objects, and at all angles. The elicitation of this result, which was quite clear-cut, was the object of the experiment. Other findings will, however, be briefly mentioned.

Of the other three main variables, angles inevitably accounted for by far the largest proportion of total variance and gave a highly significant F -ratio. This was, of course, expected from the nature of the experimental procedure and is, from the point of view of this experiment, quite unimportant.

The different types of stimulus objects—disk, square and rod—had no noteworthy effect on the degree of phenomenal regression as the F -ratio for ‘stimuli’ was not significant.

There was a fairly strong tendency for women to exhibit less phenomenal regression (i.e. less perceptual constancy) than men. The F -ratio, however, just failed to reach the 0.05 level of significance.

Of the interactions, not one gave a significant F -ratio. There was a very slight suggestion of a differential reaction to the three stimulus-objects by (a) men as opposed to women, and (b) introverts as opposed to extraverts. The probability of a chance result is too high, however (P = about 0.20), for any importance to be attached to this result.

VI. DISCUSSION

(i) *Introversiion-extraversiion*

The significant relationship, which we have found to exist in our subjects, between introversion as a personality trait and reduced constancy in perception of shape, is in general agreement with other workers (Thouless, 1932; Izzet, 1934; Holaday, 1933; Klimpfinger, 1933a; Weber, 1939). It is also, we feel, in accord with non-scientific everyday experience. It is somewhat surprising that Sheehan (1938, p. 20, footnote) attached no great weight to the theory that personality might be one of the factors determining constancy.

One explanation of the non-significant results of both Thouless and Izzet may be that they were both ostensibly working with the schizothymic-cyclothymic dimension, subjects being selected on the basis of the authors’ assessments. Thouless himself (1932) specifically equates schizothymia-cyclothymia with introversion-extraversion, but we now have reason to doubt that, in their pure form, these dimensions are in any way related. Hence, it is not surprising that his results differ from ours, since he was not dealing with introversion-extraversion in as pure a form. Such correspondence as does exist between his results and ours may be attributed to the fact that, in his selection of schizothymes and cyclothymes, he was influenced by considerations of introversion and extraversion.

Weber’s method bears superficial resemblances to ours, but his results are less highly significant. Three possible factors come to mind. First, the instructions given to our subjects were such as to motivate them to perceive phenomenologically rather than in terms of the real object. Though in our experiment extraverts and introverts were presumed to be equally motivated, the latter were probably less rigidly bound to customary perceptual patterns—to perception in terms of the real object. Sheehan (1938) has commented on the apparently greater flexibility in the perceptions of subjects showing low constancy. Weber’s subjects, on the other hand, were required to perceive in terms of

a supposed real object. It appears to us that such a set is less likely to emphasise differences in personal perceptual patterns.

Secondly, Weber's subjects were American sophomores and, as a body, probably contained fewer really introverted individuals than did our group of Scottish students. Whereas the published norms show that 5% of the American population have scores of less than 85, ten of our group (11%) have scores below this level. On the other hand, at the extraverted end of the scale, no such difference was apparent. This implies that our group spread more widely over the scale; and selection of extremes may have resulted in a more marked contrast between our introverts and extraverts.

Thirdly, for our experiment, we selected as subjects twenty-four extremes out of a total of ninety-one, while Weber chose fifty out of seventy-five. It seems likely, therefore, that our introverts and extraverts were more distinct groups than Weber's and that this helps to account for the more significant results we obtained.

(ii) *The sex difference*

Our results which, though they fail to reach significance, suggest a difference between the sexes, the women showing lower constancy than the men, do not agree with those of Thouless, who found a significant trend in the opposite direction. Methods of selection of subjects, and the fact that, in his experiment, personality variables were not partialled out, may account for the disagreement. Klimpfinger (1933*b*) on the other hand, as we have mentioned, whose subjects ranged in age from 3-year-olds to adults, found that up to the age of 12, boys showed lower constancy, and, thereafter, greater constancy, than girls. There is no indication in her article of the significance or non-significance of these results, and again personality variables were not controlled, but one might say that her results, in the relevant age-groups, agree with ours.

(iii) *The learning factor in perception*

One interesting feature of our results, which just failed to be significant, was that the introverted group, and particularly the male introverts, showed a progressive movement towards the low constancy end of the scale as the experiment proceeded, while the extraverts showed no tendency in any direction. Since the order of presentation of stimuli was not randomized (each subject was shown all positions of the disk, then the square, and finally the rod), it is not possible at present to say whether this tendency is a function of the change of stimulus object or an indication of some learning process at work. An experiment, designed to answer this question, is in progress.

(iv) *Theory*

Before attempting to formulate any hypothesis to account for differences in constancy, it might be profitable to review briefly some of the main conditions and cases where low constancy is known to occur. These include (a) low illumination, (b) the use of a reduction screen, (c) monocular vision and (d) a relatively unstructured visual field, giving few cues. In addition, we have noted (e) that a phenomenologically directed attitude (as opposed to an object-directed one) results in reduced constancy, and (f) that low constancy occurs in introverts. We have also found it (g) to be a frequent feature of mescaline intoxication.

We would suggest three possible types of reduced constancy: that due to sensory deprivation, that due to reduced attention to the environment, and that due to a failure in the organizational process. The first calls for little comment; (a) to (d), mentioned above, are obvious examples.

A reduced attention to the environment appears the most plausible explanation of the reduced constancy we have been discussing in this paper. Though superficially similar to the 'sensory deprivation' type, because of the consequent reduction in amount of information being fed in, reduced constancy due to inattention may be more akin, from the point of view of neurological function, to the third type, presently to be mentioned. Experimental investigation of this second type of reduced constancy might well be carried out using 'latent learning' methods, or by 'attention-saturating' techniques, with particular emphasis on its relationship to speed of learning.

The third type of reduced constancy—organizational failure—is, we tentatively suggest, exemplified in mescaline intoxication. A hypothesis along these lines has already been presented elsewhere (Ardis & Drewery, 1954). Therein, the concepts of Hebb (1949) were applied to certain phenomena of mescaline intoxication. The hypothesis may briefly be summarized thus:

The neuronal basis of perception is taken to be the activation of assemblies and phase-sequences of neurones. Activation, inhibition and modification of these complex spatio-temporal units are regarded as determined by (a) the nature of the stimulus and the resultant input to the central nervous system, and (b) stored experience in a neuronal form. The latter might conveniently be labelled a 'reality-scheme', analogous to the 'body-scheme' postulated by neurologists, and regarded as a vastly complex—and continually developing—type of phase sequence. The interaction between (a) and (b) might be expressed either as the shaping—by facilitation or inhibition—of the crude response to (a) by (b); or as the activation of fractions of (b) by (a). The distinction is verbal.

It is suggested that the interaction is disorganized by mescaline. One consequence of this may be the emergence of a more primitive, sensory-dominated, perception—one example of which is reduced constancy.

With other effects we are not at present concerned. But it may be suggested that mescaline disorganizes maximally those neuronal units which are the basis of complex, recently learned, responses. This subject may also be viewed from the standpoint of learning theory. A paper (Ardis & Drewery) along these lines is in preparation.

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VISUAL DISCRIMINATION OF ORIENTATION BY OCTOPUS

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Recent research on visual discrimination has largely ignored one of the major problems in this field, namely what are the properties of shapes to which animals are responding when they discriminate them: a solution to this problem must precede attempts to arrive at the mechanism behind shape discrimination. Experiments were conducted on the discrimination of orientation by octopuses. It was shown that discrimination of orientation was easy where the orientations to be discriminated fall in the vertical and horizontal directions, extremely difficult where the orientations are midway between the vertical and horizontal directions. Various transfer and memory tests were carried out which confirm that the animals were discriminating orientation and not other properties of the figures. A tentative explanation of the results is proposed. An analysis of serial effects is attempted which suggests a possible interpretation of some of Boycott & Young's findings on the effects of lesions to the verticalis lobe in octopus.

INTRODUCTION

In recent years many experiments have been undertaken which involve the learning of visual form discriminations by animals: this experimentation, however, has tended to neglect one of the most important aspects of visual form discrimination. By using situations involving such discriminations recent experimenters have for the most part attempted to solve problems of three different kinds: (1) Problems related to the speed of learning when the stimuli to be discriminated are arranged in different ways in relation to the response which will reveal the discrimination, e.g. the successive *v.* simultaneous discrimination issue (Bitterman & Wodinski, 1953), or the approach-avoidance issue (Nissen, 1950). (2) Problems arising from the continuity non-continuity dispute (Blum & Blum, 1949). (3) Neurophysiological issues relating to the site of the centres involved in discrimination in the brain, the specific pathways involved, etc. (Sperry, Miner & Myers, 1955). There is a further problem which in recent years has largely been ignored: this is the problem of arriving at the actual mechanism by which the discrimination is performed. Logically, this question is independent of any of the other three types of question. It is concerned with the way in which visual information is sorted. Questions of type (1) are concerned with the ease with which a given sorting mechanism can be built in or become effective with variations in the ways in which the response must be attached to the stimuli to be discriminated. Experiments of type (2) attempt to answer questions such as, given that an animal has learned to respond in terms of information sorted in one way, how far does this impede or assist the animal in switching to a different method of sorting. Experiments of type (3) have led to some knowledge of the whereabouts of the centres concerned with sorting in the brain: they have given us little information so far about what the sorting mechanism itself is like. One reason for this may be that the functional organization of nerve cells in the brain does not correspond in any simple fashion to their spatial organization: thus comparatively macroscopic lesions may tend to destroy not simply all cells having a similar function in the total organization but cells having a great diversity of function. Thus none of these approaches is specifically designed to throw light on the sorting mechanism *per se*: indeed, for answering questions about the sorting

mechanism itself it may not matter whether the sorting mechanism only comes into being through learning or whether it is largely innate—we might be able to answer the question what kind of sorting mechanism was operating in a given animal at a given time without knowing how far it was innate, how far it was learned.

A first requisite for arriving at some idea of what the actual sorting mechanisms used are, is to know what are the properties of the stimuli being discriminated to which the animal is responding when it makes the discrimination. It is also important to know which stimuli the animal finds easy to discriminate and which are difficult for it to discriminate. The explanation of why certain properties are seized upon and not others, and why certain discriminations are easy while others are difficult or impossible, must lie in the peculiar properties of the sorting mechanism. If we know sufficient facts of this kind, we may be able to construct a hypothetical sorting mechanism or mechanisms which will explain the facts. Unfortunately, at the moment we know extremely few of these facts, and nearly all that we do know are due to the pioneer work of a few investigators in the 1930's, and in particular to the classic studies of Lashley (1938) and Fields (1932). There has been little systematic work of this kind in recent years, and the work reported in this paper is intended as a beginning in an attempt to follow up the data assembled by Lashley and others in the thirties, in the hope that by accumulating such data it may eventually be possible to put forward concrete suggestions as to the sorting mechanisms operating in visual discriminations.

In so far as any one factor emerges clearly from the investigations of these problems with the rat in the jumping box conducted in the thirties it is that in this situation one of the properties of figures most readily responded to is that of orientation. Thus Lashley found one of the easiest discriminations for the rat to learn was between vertical and horizontal striations; a rat can distinguish between a triangle on its base and a triangle on its apex more readily than between a triangle and a square; nearly all pattern discriminations are upset by rotations of the figures. If we examine Lashley's evidence carefully, however, a second feature seems to emerge in connexion with discrimination of orientation—this is that discrimination of figures on the basis of their orientation is particularly easy where the main lines of the figures to be discriminated run respectively in the vertical and horizontal directions (vertical versus horizontal striations). Where, however, the two figures to be discriminated are at 90° to one another but do not fall in the vertical or horizontal plane, there is evidence that discrimination may be difficult or impossible. Thus no rat learned to discriminate N from M or S from Z: now the parts of these pairs which differentiate one member of each pair from the other are the lines $\backslash v. /$ and $\backslash v. \int$. In each case these are identical lines moved round through 90° , but falling midway between the vertical and horizontal directions. Although this evidence is suggestive it is not conclusive: Lashley himself explicitly states that the rat finds no difficulty in discriminating mirror images as such, though Deutsch (1955), apparently relying on Lashley, claims that mirror images are particularly difficult to discriminate. In fact, Lashley's rats had little difficulty in discriminating other pairs of mirror images (e.g. upright versus inverted triangles, upright versus inverted U shapes, etc.).

The present series of experiments is an attempt to test for one lower animal the hypothesis that shapes tend to be classified in terms of their orientation, and that in this classification the extent to which they can be directly related to true vertical and horizontal coordinates is important. There are several reasons why the rat is not a very suitable

animal for this study: (1) Lashley found that his rats have a preference for horizontal stripes *v.* vertical stripes, and indeed of his rats which were being trained positive on the horizontal stripes, over half of them learned the discrimination without a single error. Though Lashley himself does not speculate, this may be because in making a jump a rat could normally be jumping to a horizontal surface. Whatever the reason, it complicates the interpretation of these results. (2) Moreover, the rat in the jumping box has been found to tend to respond in terms of parts of the figure and particularly in terms of the lower parts of the figure only (e.g. Munn, 1930), and this again makes results difficult to interpret. (3) In the jumping stand it is difficult to prevent the main lines in the field running in a vertical or horizontal direction: it is seldom clear whether it is the figures themselves which are being discriminated or the figures in relation to the surrounding field (Lashley, 1938).

For these reasons it was decided to conduct the present investigation on octopuses, and so because it was hoped to connect up the writer's own results with the data accumulated on form discrimination and brain lesions in octopus by Young and Boycott. The purpose of this series of experiments then is threefold: (1) To investigate the way in which octopus discriminates orientation, which seems to be one of the most important properties of shapes for discrimination by lower animals, with a view to uncovering something of the mechanism behind discrimination. (2) To compare shape discrimination in the octopus with shape discrimination in the rat. (3) To investigate certain other parameters affecting the behaviour of octopus in the situation used; many of these parameters are also being currently investigated by Young and Boycott, and without a knowledge of their effects it would be impossible to evaluate the data on shape discrimination in the octopus fully. The investigation itself will be reported according to the following plan: (1) An account of the experimental situation and experimental methods used by the present experimenter, including a short account of the habits of the octopus in so far as these are relevant to an understanding of this series of experiments. Fuller accounts of the habits of the octopus will be found in Boycott (1954). (2) An account of the separate experiments conducted and a summary of the results. (3) Discussion of the results.

I. THE EXPERIMENTAL SITUATION

Octopuses were obtained fresh from the sea: they weighed about $\frac{1}{2}$ kg. at the start of the experiments. They were kept individually in separate tanks made of Eternit (a pressed asbestos substance, light grey in colour). The internal diameters of the tanks were about 90 cm. long by 60 cm. wide by 53 cm. high. They were filled with water to a depth of about 37 cm. On the inside the tanks were rounded at all the junctions of sides. At one end of the tank, a 'home' was provided for the octopus made of three bricks against the middle of one short wall of the tank. The octopus spends most of its time sitting in the hole formed by the bricks, looking out down the length of the tank. The tanks were covered by an Eternit lid, which had a hole 25×10 cm. cut in it at the end away from the octopus's home to admit light.

Pretraining

The octopuses were given 4 or 5 days in the tank before training started, to allow them settle down in their new environment.

They were then trained to feed: the lid of the tank was pushed back to a distance of about 25 cm. from the end of the tank opposite the octopus's home, and a crab was thrown in. Though many animals attack the crab readily from the start, particularly if they have been left for several days in the tank without food, others take 10 or 20 sec. to attack at first. Animals were fed five crabs a day in this way at 2 hr. intervals until they reached a criterion of four out of five attacks on 1 day within 5 sec. (Further information on this question will be found in Boycott & Young, 1956*b*). Animals normally reached the criterion of attacking within 2 or 3 days from commencing feeding. Animals which failed to do so were rejected.

When each member of a given group of animals had reached the criterion, the octopuses were then presented with a crab on the end of a holder. The holder was an aluminium tube 7 mm. in diameter and 28 cm. long. This method of presenting the crab was used in preference to the method of Young and Boycott (that of attaching the crab on the end of a piece of cotton and dangling it), because it enables the experimenter to put the crab in contact with the octopus more quickly after the octopus has attacked the positive shape. In fact, it seems doubtful if the slight delay in reinforcement with the method of presenting the crab on the end of a thread makes any difference to the octopus's performance. All octopuses readily attacked the crab on the end of the holder, and only one day's trials were given of this (five trials in all).

Two days' training was then given with the positive shape only, with crab reward. Two animals which failed to attack the positive shape regularly on the second day were discarded.

Positive trials

At this stage and throughout all subsequent stages of the experiments (unless otherwise stated) the method of presenting the positive shape was as follows. Lids were always pushed back about 25 cm. from the end of the tank a few minutes before the experiments began, to give the octopus a chance to adapt to the stronger light. The shapes were cut out of opaque Perspex $\frac{1}{4}$ in. sheet. Since Boycott & Young report difficulty in getting octopus to attack stationary shapes, a moving shape was used. To control the amount of movement, a motor was used to move the shape and the shape was suspended from the motor by a vertical brass rod. The motor imparted an elliptical motion to the shape; its longest movement in any direction was about 2 cm. The experimenter inserted the shape by grasping the motor, and lowering it on to the edge of the tank where it was held firm by a clamp. When in position the shape was 7.5 cm. in front of the end of the tank away from the octopus and the middle of the shape was 14 cm. above the bottom of the tank. The shape was not inserted unless the octopus was in his home at the time or immediately in front of his home. This meant that the shape was presented in front of the octopus, at a distance of about 70-85 cm. from its eye, and slightly above eye level.

The criterion of attack was that the octopus should seize the shape sufficiently firmly to stop the motor revolving within 30 sec. from the entry of the shape into the water. As soon as an octopus had seized the positive shape in this way, it was rewarded with a crab presented directly to the underside of its arms on the end of a holder. If an octopus did not attack within 30 sec., the crab was inserted on the end of the holder and waved about slightly in the neighbourhood of the shape. By keeping the crab slightly behind the shape it was possible to ensure that the octopus did not catch the crab without attacking the

shape as well. Normally an octopus which had not attacked the positive shape would attack immediately on being presented with the crab.

Negative trials

In negative trials the shape was introduced in the same manner. If the animal attacked within 30 sec. a shock was administered to the mantle with a shocking device devised by Boycott & Young (1956*b*). This consisted of two electrodes about 1 cm. apart on the end of a long probe. Alternating current at between 6 and 10 V. was used. The electrodes were left in contact with the mantle until the animal released the shape—this usually happened instantaneously. The negative shape was left in for 30 sec. even if it had already been attacked. If the animal made more than one attack within 30 sec. it was again shocked: in fact more than one attack was extremely rare except within the first few presentations of a negative shape.

Training

After pretraining on successively (1) crabs alone, (2) crabs attached to holder, (3) positive shapes, as described above, animals were trained on a discrimination and subsequently tested for various kinds of transference. The procedure on any day which involved training (or retraining) was as follows; five pairs of trials were given a day, at approximately 2 hr. intervals. Each pair of trials consisted of one presentation of the positive shape and one of the negative shape: about 15 min. were left between the two trials making up a pair of trials. On any one day three pairs of trials would have the negative member first, the positive second. Five different orders for pairs of trials were used so that the order of the pairs in which positive came first or in which negative came first varied from day to day: thus the order for the first day (with commas to denote each pair) was $+-, -+, -+, +-, -+$. In the orders of pairs used no pair of trials with the positive member first was followed by a second pair with the positive member first, and not more than two pairs with the negative member first were placed together. This did not hold good of the order from one day to the next. At the end of each 5 days of training or retraining the order of trials went back to that on the first day. Trials were given in pairs in this way partly for the experimenter's own convenience, partly to make the results comparable with those of Boycott & Young. The effects of this method of presentation and the time intervals used will be discussed later.

Tests

A second procedure was also used at stages throughout the experiments—this we shall refer to as presenting tests. On a day where tests were being given pairs of shapes were presented as above but no reward or punishment was given. On test days the shapes were simply presented to the octopus and removed at 30 sec. or, if an octopus attacked earlier, removed when attacked. We shall refer to the pairs of test shapes used as being positive or negative, though since they were usually shapes not seen by the animal before, and since the animal was not differentially trained on them during test days, they were then not differentially reacted to by the octopus. The order of presentation for all test days was $+-, -+, +-, -+, +-$ for half of any group of animals and $-+, +-, +-, -+, -+$ for the other half. The half of the group getting one order on one test day would get the opposite order on the next test day. On test days, each octopus was

fed one crab a quarter of an hour after the second presentation of each pair of presentations.

On certain other days during the experiments the animals were allowed to rest: the animals were fed their quota of five crabs on those days.

Throughout the experiments an effort was made to give trials and tests at approximately the same times each day for any animal: for the first animal the times were 10.30, 12.30, 2.30, 4.30, 6.30. Trials were never given at less than 1 hr. 50 min. from one another, but sometimes more than 2 hr. was allowed to elapse between trials in which case the later trials of the day would be correspondingly delayed.

The experiments were conducted during the months of July, August and September and during this time the water in the tanks remained at a temperature of $22^{\circ}\text{C} \pm 1$. The crabs used weighed between 8 and 16 g.

In the presentation of the experiments and the results which follow it will be seen that the number of animals used occasionally varies from day to day. This is because during a period of 3 months over which the experiments extend, animals occasionally became unwell. There were two chief causes of ill-health on the part of the octopuses: (1) Octopus climbing out of the tank (often despite the fact that the lid was well secured) and falling on the laboratory floor. (2) Failure of the jets of running sea water in a given tank. In both cases the behavioural effects are a great reduction in the tendency to attack: the scores of octopuses who were unhealthy for one or other of these reasons are not included in the following report for the period over which they remained unhealthy.

Significance of results

The significance of results was tested for in two ways. To find whether any two shapes were differentially reacted to in a series of trials, a χ^2 test was applied to see whether the ratio of number of attacks on the positive shape: failures to attack positive differed from the ratio of attacks on the negative: failures to attack the negative. To find whether discrimination in any given series of tests (A) was significantly better or worse than in any other given series (B), a χ^2 test was applied to the ratio errors made in series A: successes in series A, compared with the same ratio for series B. In all cases Yates's correction for small numbers was applied.

II. THE EXPERIMENTS

There were two major experimental groups of animals. In stage I, group A was trained first to discriminate between a vertical and a horizontal white rectangle 10×2 cm.: animals in this group then received training in discriminating rectangles of the same shape but running at 45° to the vertical and horizontal and at 90° to one another. Group B received the same training, but in the reverse order. In stage II various transfer tests based on the discrimination between vertical and horizontal were carried out, with retraining on the original horizontal versus vertical discrimination interspersed amongst the tests. Finally an attempt was made to train the animals to maintain the original discrimination when stationary shapes were used and the rod was removed from the situation. Tables 1 and 2 summarize respectively stages I and II of the experiment. In presenting the results the order in which training and tests were given will not be followed. There were six animals in group A and four animals in group B.

Table 1. *First stage*

		Results		
Day	Training	No. of trials	Attacks on pos.	Attacks on neg.
Group A				
1-5	Pretraining	—	—	—
6-12	Training on 10×2 rectangles, horizontal <i>v.</i> vertical	210	183	54
14-15	Pretraining on positive oblique	—	—	—
16-35	Training on 10×2 rectangles obliques	600	332	238
39	Memory test 10×2 rectangles, horizontal <i>v.</i> vertical	30	25	5
40-42	Retrain, horizontal <i>v.</i> vertical	80	68	10
43-44	Training, horizontal <i>v.</i> vertical. Stationary shapes	60	46	11
45	Retrain, horizontal <i>v.</i> vertical	30	25	6
Group B				
11-15	Pretraining	—	—	—
16-35	Training on 10×2 rectangles obliques	400	194	155
38-39	Pretraining on horizontal <i>v.</i> vertical. Positive only	—	—	—
40-45	Training on 10×2 rectangles, horizontal <i>v.</i> vertical	90	81	24

(1) *Training on the vertical against horizontal discrimination*

This discrimination was between a 10×2 cm. white vertical rectangle and a similar horizontal rectangle. It was mastered very rapidly by both group A, which learned it first, and by group B, which learned it after 20 days' training on the oblique discrimination. Fig. 1 gives a graph of the results. The figures for group B are based on three animals

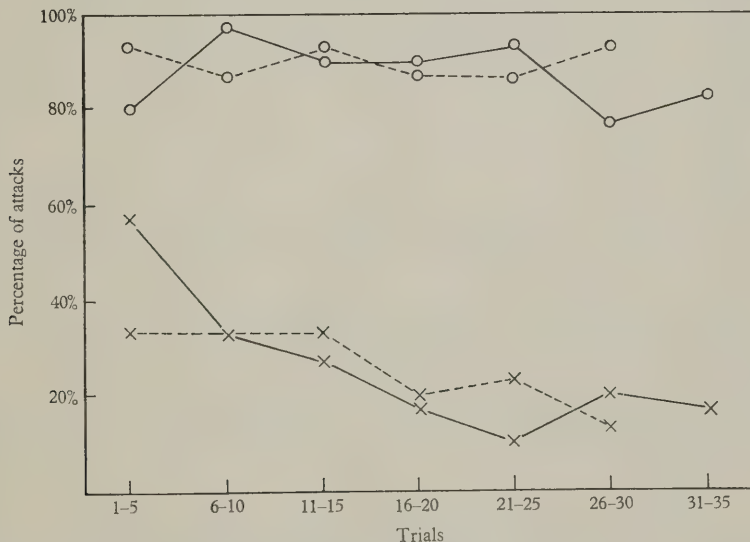


Fig. 1. Learning of vertical v. horizontal discrimination. —, group A; ----, group B; ○, attacks on positive; ×, attacks on negative.

ly, since one animal from this group escaped from the tank half way through training. Half of the animals in each group were trained positive to the vertical rectangle, half to the horizontal rectangle. There is no evidence that difficulty in learning varied with which rectangle was made positive or negative. Thus, in group A over 7 days, the group trained with horizontal positive made 87 attacks on the positive figure and 27 on the negative, whereas the animals trained with vertical positive made 96 attacks on the positive figure

and 27 attacks on the negative. It will be noticed that neither group A nor group B became perfect in the discriminations—mistakes were always likely to recur. Although group A received retraining on this discrimination on 15 subsequent days, and group B on 11 days, there was no sign of any general improvement in performance—performance throughout remaining around the level which it had reached on the third or fourth day of training.

(2) *The oblique discrimination*

Training on the oblique discrimination was carried out with the same size rectangles as training on vertical versus horizontal. Half the animals of each group were trained positive on the rectangle sloping one way, half were trained positive on the other. Neither group A (trained on the oblique discrimination after mastering the vertical horizontal discrimination) nor group B (trained on the oblique discrimination first) mastered the discrimination fully. Results are presented graphically in Fig. 2.

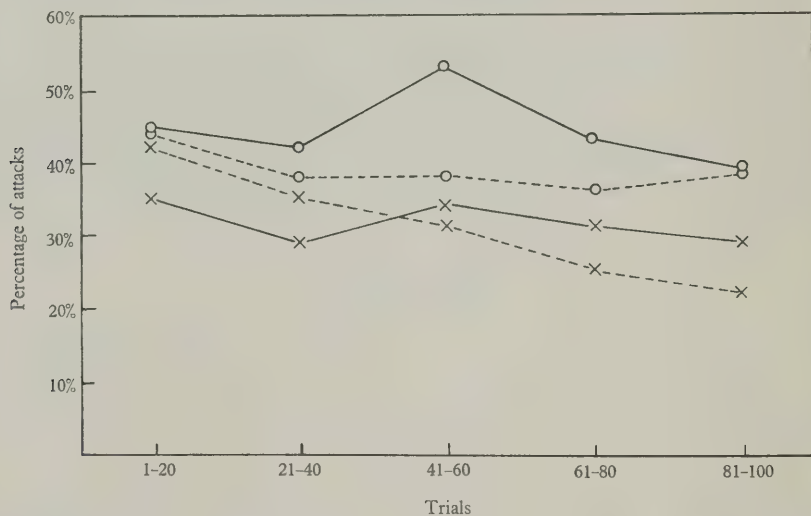


Fig. 2. Learning of oblique discrimination. —, group A; ---, group B; ○, attacks on positive; ×, attacks on negative.

The difference between the learning of the horizontal versus vertical discrimination and the learning of the oblique discrimination is highly significant. For example, when either group A's or group B's performance on the first 2 days' learning of the vertical versus horizontal is compared with either group A's or group B's performance on the first 2 days' learning of the oblique discrimination, the four χ^2 values obtained range from 7.1 to 11.5 with one degree of freedom. Thus the original hypothesis that discrimination of identical figures in different orientations is facilitated when these orientations fall in the vertical and horizontal planes, and is difficult when orientation falls midway between the two is confirmed.

It will be noticed that both for groups A and B the discrimination learned second seems to be learned faster than the same discrimination learned first. Actually the differences on the vertical-horizontal habit are not significant (χ^2 for the first day of A versus the first day of B learning the vertical versus horizontal habit is only 1.7, and for the first 2 days combined is 2.3). The differences on the oblique discrimination become

nificant only for the first 12 days of learning (χ^2 3.9). What evidence there is suggests that learning of one discrimination slightly facilitates learning of the other.

Individual differences in performance on the oblique discrimination were very considerable; three animals gave no evidence of learning at all, and two animals which had achieved beyond chance scores during the first 10 days make almost an equal number of attacks on the positive and negative figures during the second 10 days. This falling off in discrimination in a difficult habit has been noticed by Wells & Wells (1956) with tactile discrimination in the octopus.

(3) *Further experiments with original vertical versus horizontal figures*

(a) *Movement.* It may be suggested that although movement was controlled by a machine, the original vertical versus horizontal discrimination may have been based on slight differences in the motions of the figures due to their differential resistance to water pressure when moving in different directions. To control this possibility group A were retrained on days 43-44 with stationary shapes: the shapes were presented as before, but the motor was not started. The scores produced on these two days (46 attacks on the positive, 11 attacks on the negative out of 60 presentations) do not differ significantly from scores on days when the shapes were in motion.

(b) *Presence of the vertical rod.* It is possible that the vertical versus horizontal discrimination was being performed by the animals by reference to the vertical rod, at the end of which the shape was attached. However, controls used rendered this unlikely. Thus substitution of a transparent Perspex rod for the original brass rod did not affect the discrimination. In addition, an attempt was made to see whether the animals would continue to discriminate the vertical from the horizontal when the rod was no longer present in the situation. The positive and negative shapes were presented one at a time, attached to a board of the same size as the far wall of the tank against which the shapes were normally seen and made of a grey Eternit substance similar to the walls of the tank. This board was lowered vertically into the tank, just inside the wall of the tank away from the octopus's home, and when it came to rest at the bottom the shape was in the same position in the tank as it had been when presented on the end of a rod. One animal continued to discriminate (8 attacks on the positive, 1 on the negative during the first 10 pairs of trials), but the other animals showed reduced tendency to attack, or attacked the board rather than the shape. It seems likely that the size of the board may have been producing fear in some of the animals (colour changes associated with fear were frequently observed), and also the shape was no longer treated as an independent object but as part of the board with which of course it moved as the board was lowered into the tank. Despite this some discrimination was continued: the score on the first day was 13 attacks on the positive and 3 on the negative out of 45 trials with each (9 animals), on the last days together 34 attacks on the positive and 12 on the negative out of 60 trials with each (5 animals). This indicates at least that the vertical rod is not an essential cue in the discrimination of vertical versus horizontal.

(4) *Memory*

Boycott & Young (1955, 1956*a*) suggest that if the shapes learned are not shown, some forgetting of a discrimination may occur comparatively rapidly in octopus. One test of retention over a period of time was included in the present series of experiments. On day 39

group A was tested for retention of the vertical horizontal-discrimination, after a period of 27 days without being called on to make this discrimination. The horizontal and vertical rectangles were those that the animals had been trained on. The score for 6 animals was 25 attacks on the positive and 5 on the negative out of a total of 30 presentations in each case. Although no reward or punishment was given, this in fact is the same score as on the last day of training with these shapes. It may be, of course, that intervening training on the oblique discrimination had helped to preserve the memory for the original horizontal-vertical discrimination. Since, however, the oblique rectangles were both exactly midway between the horizontal and vertical positions, this is unlikely.

(5) Transfer tests

The procedure employed during transfer tests has already been described. Each transfer test was always preceded by at least one day's retraining on the original discrimination. Since in the original training half the animals were trained positive to the vertical, half to the horizontal shape, in transfer tests where positive results were obtained the positive of the two transfer shapes for any animal was determined by the shape to which it was originally trained positive. Transfer was tested with seven different pairs of shapes. To guard against the possibility that the animals were becoming 'experimentally wise' on days when transfer tests were given, the same shapes were used on two separate days—once towards the beginning of the transfer tests and once towards the end. The similarity of the scores obtained for the two days indicates that other differences amongst results in transfer tests cannot be explained in this way (see Table 2, transfer test 2 and transfer test 7).

Table 2. *Second stage, transfer tests*

Figures used	No. of trials	Attacks on pos.	Attacks on neg.	χ^2 for significance of discrimination	χ^2 for significance of difference from normal discrimination
1. Black 10×2 rectangles, vertical v. horizontal	45	36	32	N.S.	16.9*
2. 7×2 rectangles, vertical v. horizontal	45	36	3	*	N.S.
3. 20×4 rectangles, vertical v. horizontal	45	36	4	*	N.S.
4. 5×1 rectangles, vertical v. horizontal	40	23	11	6.2*	13.6*
5. 4×2 rectangles, vertical v. horizontal	40	22	13	3.3	12.9*
6. Equilateral triangles, side 10 cm. One side horizontal v. one side vertical	45	21	17	N.S.	16.2*
7. 7×2 rectangles, vertical v. horizontal	40	31	5	*	N.S.
8. 7×1 rectangles, vertical v. horizontal	40	28	8	18.2*	N.S.

A χ^2 value of 3.8 is significant at the 0.05 level of confidence.

N.S., not significant.

* Significant.

N.B. (1) Each test day was preceded by at least 1 day's retraining. (2) The first column of χ^2 values indicates the extent to which the difference in number of attacks on negative and positive shapes for any given test day is significant. (3) The second column of χ^2 values indicates whether there is a significant difference between number of errors made on any given test day and errors made on the preceding day's retraining. (4) Where χ^2 values are less than 2.0 or greater than 20.0, exact values are not shown.

The results of the transfer tests are summarized in Table 2. Two statistics were computed on the scores for each transfer test: the first column of χ^2 values gives the result of an analysis conducted to see whether the two shapes were differently treated by the animals in any given test. The second column gives the significance of the difference

tween number of errors made in a transfer test and number of errors made on the day of training preceding each test. Scores on days of retraining did not vary significantly from one to another—if the retraining day when most errors were made (18 out of 90 trials) is compared with the day when least errors were made (9 out of 90 trials), the significance of χ^2 is only 2.8.

The conclusions to be drawn from the transfer tests may be summarized as follows (numbers in parentheses indicate the number of the transfer test referred to in Table 2): Where white rectangles of different sizes from the originals were substituted, some transfer was found in all cases. Transfer was very much better for rectangles four times the area of the originals than for rectangles one-quarter the area of the originals (3, 4). Perfect transfer occurred where the ratio of long to short sides was altered from 10:2 to 2 or 7:1 (2, 7, 8): although some discrimination remained, transfer was significantly less where the ratio was altered to 4:2 (5), though in this case some of the reduction in transfer may be due to the lesser area of the figures (cf. 4). It seems unlikely that the relatively poor transfer to smaller figures can be explained in terms of visual acuity, since where there was little or no discrimination (as in the case of the oblique figures and the small figures under discussion) the octopus would launch its attack from a position much closer to the figure than where there was good discrimination: also octopuses seem to have difficulty in identifying pieces of fish much smaller than the smallest figures used here. From the results of the transference tests to figures of different sizes, it can be concluded that the octopuses were not discriminating on the basis of the relationship between the rectangles and the immediate surround. Thus they could not have been reacting on the basis of the distance between the lower edge of the rectangle and the bottom of the tank, since the lower edge of the *horizontal* 20×4 rectangle is the same distance from the bottom of the tank as the lower edge of the *vertical* 4×2 rectangle and yet they react differentially to these forms (3, 5). A similar argument shows that the octopus is not reacting in terms of the absolute lateral extent of the rectangles—the lateral extent of the *horizontal* 4×2 rectangles is the same as the lateral extent of the *vertical* 20×4 rectangle. Thus these transfer tests establish clearly that the octopus is reacting to the ratio between long and short sides of the rectangles and is not discriminating in terms of any one absolute distance involved in the situation.

The test with equilateral triangles (6) indicates that the octopus is not discriminating merely on the orientation of one long side, since there is no transfer from vertical and horizontal rectangles to two triangles, one with a side in the horizontal plane, the other with a side in the vertical plane.

As is the case with rats, there is little or no transfer when black forms are substituted for the original white ones (1).

(6) *Simultaneous discrimination*

After the transfer tests had been given, animals were retrained to discriminate between the original vertical and horizontal rectangles without the presence of the rod. This training is described in (3b) of the present section. It was thought worth while to follow this training to respond differentially to rectangles presented one at a time on the board, with training in which both rectangles were presented simultaneously on the board. Animals were shocked if they touched the negative shape, and fed a crab if they touched the positive shape. Six animals were used at this stage of the experiment and training

lasted for 2 days. (Shapes were presented to the left and right of the board at the same height that shapes had been presented when given singly.) This stage of the experiment lasted 2 days, making a total of 50 trials. On 26 trials animals attacked only the positive shape, on 9 trials they attacked either the negative shape or both shapes together. Three animals did not touch the negative shape throughout. Thus it seems that octopuses, having learned differential reactions to two shapes presented separately, may be able to transfer the reactions to the same shapes presented simultaneously.

(7) *Sequential effects*

It might be thought that in the original horizontal versus vertical, and oblique discriminations, the octopuses may have been learning to attack or not attack the second shape of a pair on the basis of what had happened with the first shape of the pair. It will be remembered that the presentations of the positive and negative members of a pair were separated by only a 15 min. interval. An octopus could in theory learn that if it had had a crab in connexion with the first member of a pair, then it must avoid the second member of a pair; if it had not had a crab in association with the first member of a pair then it was safe to attack the second member of a pair. That this was not the basis of the discrimination is proved for the vertical versus horizontal discrimination, by the fact that in the memory tests and in certain transfer tests animals produce scores as high as during retraining despite the fact that no reward or punishment was given in connexion with each member of any pair. To prove that the discrimination which appeared in the oblique discrimination was not based simply on making reactions to the second member of a pair conditional on whether or not the first member had been rewarded with a crab, it is necessary to analyse the results and show that the different number of attacks on negative and positive is not based wholly on a difference between numbers of attacks on the second members of a pair. These results are presented in Table 3. It will be seen that discrimination is as good for the first members of a pair as for the second members of a pair though total number of attacks are reduced for both negative and positive shapes when these were the second members of a pair. A full discussion of the analysis given in Table 3 will follow in the Discussion.

Table 3. *Breakdown of scores on oblique discrimination*

(Figures represent percentage of attacks.)

	%		%
Total positive	53	Total negative	39
First positive	61	First negative	43
First positive before attack on negative	81	First negative before attack on positive	48
First positive before no attack on negative	50	First negative before no attack on positive	38
Second positive	47	Second negative	34
Second positive after attack on negative	53	Second negative after attack on positive	46
Second positive after no attack on negative	43	Second negative after no attack on positive	15

III. DISCUSSION

Discrimination of orientation

The results show that the octopus can discriminate much more readily between a vertical and a horizontal rectangle than between two opposite oblique rectangles at 45° to the vertical and horizontal. The difference in ease of discrimination does not depend upon

differences of movement nor upon the presence of the vertical rod in the situation. Transfer tests have eliminated the possibility that the octopus is discriminating on the basis of the immediate relation between the forms and their background, and have shown that the octopus is discriminating the different ratios between horizontal and vertical extent produced by the vertical and horizontal rectangles.

Although the immediate background of the forms has been excluded as providing a frame of reference in terms of which the octopus might make the discrimination, the possibility that it is making the discrimination with reference to some wider property of the background has not been excluded: the vertical axis is clearly marked in the situation used by a number of asymmetries between top and bottom of the tank (direction of light, presence of floor of tank at bottom, water surface at top, etc.). It is possible to specify the difference between the vertical and horizontal rectangle in terms of the vertical co-ordinate alone—it is not possible to specify the difference between the two obliques in terms solely of this co-ordinate: thus if we wish to measure the angle of tilt of the two oblique rectangles from the vertical plane, in order to distinguish between them we must specify whether the angle is to be measured in a clockwise or anticlockwise direction and this involves reference to a second co-ordinate, e.g. to left and right on the horizontal co-ordinate. It is of course also possible that the vertical axis is marked by information from statocysts in a way in which the horizontal axis is not marked. Since it is extremely difficult (and perhaps impossible) to remove all indicators of the vertical axis from the situation, experiments to discover whether the difference in ease of discrimination between the two pairs of rectangles would disappear if the vertical axis were not clearly demarcated have not been performed. In fact, whether the vertical and/or horizontal axes are of importance because of the arrangement of the external environment, or because of the arrangement of the octopus's nervous system may not make much difference to the type of system that underlies the classification of forms: if the environmental difference is important, it must itself be reflected somewhere in the octopus's nervous system.

It is now proposed to fulfil the promise made earlier in this paper and show how behavioural evidence may enable us to put forward hypotheses concerning the system responsible for classifying shapes. Two alternative hypotheses are put forward to explain the present results: neither is developed in detail since at the moment it is impossible to choose between them, and the writer hopes to develop them in detail in a subsequent paper on the basis of further evidence. A crucial test for distinguishing between the two hypotheses is advanced.

(1) One hypothesis is that the octopus's nervous system analyses the lateral extent of forms, while preserving the position of the parts of the form on the vertical axis. This would mean that points on the vertical axis had a specific representation in the nervous system: this specific representation, if not innate, is likely to have developed as a result of learning since throughout the animal's life there are present clear and consistent differences in the environment (direction of light, etc.) corresponding to differences of height: this is not the case for differences along the horizontal axis. When a horizontal rectangle is analysed in this way, strong excitation (corresponding to its lateral length) would be produced along a small band of the points in the nervous system representing differences along the vertical axis: when a vertical rectangle is analysed in this way weak excitation (corresponding to its small lateral extent) would be produced over a large band of the points representing differences along the vertical axis. The two oblique rectangles would

produce identical excitation patterns since both their lateral and vertical extents are the same. This system would account for the difference in ease of discrimination found, and is easy to translate into neural terms. In order to perform the observed transference to rectangles of different sizes, all that is necessary is that instead of analysing the absolute amount of excitation produced by the lateral extent of the figure, this amount is analysed relative to the total area of the figure.

(2) The other possibility is that the octopus's nervous system analyses both lateral and vertical extent of the figures. In other words that both lateral and vertical positions in the visual field have specific representations. Such a system would again discriminate readily between vertical and horizontal rectangles (vertical—strong excitation over narrow band of points on lateral axis, weak excitation over broad band of points on vertical axis; horizontal—the reverse of this), and again it would give the same output for both oblique rectangles (medium excitation over a medium band of points both on the lateral and vertical axes). The second system would perform the size generalizations more easily than the first, since all that would be required is that the amount of excitation over any given range of points on the vertical and lateral axes be compared: if it is stronger on the lateral axis the rectangle is vertical, if on the vertical axis the rectangle is horizontal.

A crucial test for the two systems is as follows: the second system should discriminate equally well between a pair of T figures orientated $\perp$ and T and a pair orientated $\dashv$ and $\vdash$: the first system should discriminate much more readily between the former pair than the latter. Lashley, in fact, found rats discriminated more readily between U figures differing in the vertical axis (upright and inverted) than between U figures differing in the lateral axis (figures lying on opposite sides): but because of the rat's habit of discriminating in terms of the base-line of the figure, this finding is difficult to interpret.

The fact that octopuses can with much training discriminate slightly above chance between the oblique rectangles does not contra-indicate these hypotheses: it would be possible to perform this discrimination on the systems proposed if the octopus first learned to fixate a given part of the figure. One of the advantages of both systems is that they will explain recognition of many orientations independent of point of fixation. These hypotheses, and particularly the second, are capable of explaining many facts of form vision: they will be developed in more detail, if further experimental testing seems to warrant it.

Fluctuations in tendency to attack

The reduction in the number of attacks made both for positive and negative shapes when the shapes were presented as the second members of a pair as compared with numbers of attacks made on the first members of a pair requires some explanation. The figures on which the following analysis is based are presented in Table 3. They are figures for all animals learning the oblique discrimination. Similar trends appear, however, in the training, retraining and testing with the vertical and horizontal shapes, but the trends there are not so pronounced since they are overshadowed by the effects due to good discrimination of the vertical and horizontal figures.

The situation is complicated because tendency to attack seems to have varied from one pair of trials to another in each octopus. We can see this by comparing the percentages of attacks on the positive when the positive came first for animals who went on to attack

negative against the percentage for animals who did not go on to attack the negative. The percentages are 81 versus 50—a highly significant difference. This is not a serial effect since the two groups are selected on the basis of how they reacted to the *second* figure of the pair. The difference in these percentages represents fluctuation in some general tendency to attack. There is a similar difference in percentages of attacks on the first negative between animals who went on to attack the positive and those that did not (81 v. 38%). We would of course expect such fluctuations in general tendency to attack merely on the basis of generalization from one figure to the other apart from other considerations.

However, this fluctuation in general level of tendency to attack will not explain the reduced number of attacks on the second members of pairs of figures. The obvious explanation for reduction in number of attacks where the positive figure was second is to suppose that this is due to the reduction in attacks in octopuses who had just attacked the positive and had received a shock. To find out whether this is the case or not we must compare two sets of figures: (1) (*a*). Percentage of attacks on second positive where the first negative had been attacked, with (*b*) percentage of attacks on first positive where the following negative was attacked. (2) (*a*). Percentage of attacks on second positive where the first negative had not been attacked, with (*b*) percentage of attacks on first positive where the following negative was not attacked. We must make the comparison in this way in order to parcel out any effect due to fluctuations in general tendency to attack: thus 2 (*a*) cannot be compared directly with percentage of animals attacking the first positive, because 2 (*a*) is already selected as a group which does not attack the negative and this in itself might tend to produce a lower percentage of attacks in the second positive. Now when the comparisons are made in this way we get the following figures: (1*a*) 53%, (1*b*) 43%; (2*a*) 43%, (2*b*) 50%. The difference in comparison (1) is highly significant, that in (2) falls just short of significance ($\chi^2=3.1$). The fact that the first difference is greater than the second suggests that the shock is reducing the number of attacks on the second positive. The fact that there exists a nearly significant difference in (2) suggests that some additional factor may have to be taken into account in explaining the difference in number of attacks on the second positive from number of attacks on the first positive. This impression is strengthened when we break down the results for percentage of attacks on the second negative.

Making these comparisons for percentage of attacks on second negative figures in the same way as for the second positive to avoid effects due to general fluctuations in level of attack, we have: (3*a*) percentage attacks on second negative after an attack on positive, compared with (*b*) percentage attacks on first negative before an attack on positive. (4*a*) percentage attacks on second negative after no attack on positive, (*b*) percentage attacks on first negative before an attack on positive. The percentages are (3*a*) 46, (3*b*) 48, (4*a*) 15, (4*b*) 38. Once again percentages of attacks on the second figure are down as compared with the first, though this time they are only significantly down where the positive figure was not attacked within 30 sec. It must be remembered that even in cases where the positive figure was not attacked within 30 sec. it was attacked after 30 sec. when the crab was shown in association with it. It may have been decreases in motivation due to getting a crab that led to the difference in (4), but if this is so a similar difference could show in (3). The main difference between animals who did attack the positive first (3*a*) and animals who did not attack the positive first (4*a*) will be that the positive

shape has been exposed longer for (4a) than for (3a). All our results can be explained if exposure to a shape tends to reduce the tendency to attack a shape—in comparison (1) there will be the additional factor of the effects of shock.

Now this possibility is of considerable importance in the context of Boycott & Young's experiments on the effects of lesions to the verticalis (Boycott & Young, 1955). They have found a tendency for animals lacking the verticalis to lose discrimination and to be unable to relearn it under normal conditions. They tend to attack both positive and negative shapes. However, some of the animals can be retrained by presenting shapes at very short intervals—a condition under which normal animals either do not learn at all or are at least much retarded since they cease attacking. In an experiment not yet published, Wells & Wells present similar findings for touch discrimination in animals lacking the verticalis. Young & Boycott suggest tentatively that the negative memory in verticalis animals is very short lived and can only be kept going if the traces are strengthened by frequent presentation (every 5 or 10 min.) of the negative shape even when the negative is not attacked. The present results suggest as a further possibility that verticalis removal may raise general tendency to attack and that discrimination can only show up when this tendency is reduced, and that this reduction may be brought about by very frequent presentation of shapes.

Reality of situation

The rates of learning of the octopus in the situation used seem to compare very favourably with rates of discrimination learning for the rat. It may be that this is because the situation used (as developed by Boycott & Young) is much closer to what the animal is called on to do in its natural environment than are discrimination situations hitherto used with rats. In this connexion the following features should be noted: (1) The reaction is made directly to the shape to be discriminated: Murphy & Miller (1955) found considerable reduction in rate of learning for monkeys where the response was not made directly to the object discriminated. (2) The reaction is a natural reaction for the octopus and does not involve elaborate motor learning. (3) The reward is one that the octopus would normally get for making a positive reaction to shapes in its natural environment. (4) The shapes are the shapes of objects, not merely two-dimensional patterns. It seems likely that animals are normally called on to discriminate shapes of objects not patterns and that object discrimination may be an easier discrimination for animals to make than discrimination between two-dimensional patterns. Moreover, it may be that because the shapes are real objects (emphasized by their movement against the rest of the field) that the animals seem to transfer something learned about this shape in one context (on the end of a rod) to other contexts (successive and simultaneous discriminations with the shapes on a board).

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CORRIGENDUM

In 'Some studies of perception in relation to age' by Jean G. Wallace, Vol. 47, part 4, page 287, Table 2, under 'Group 2', should read as follows:

Age	No. successes	No. failures				No. failing
		1	2	3	4	
51-60	18	2	6	.	.	8
61-70	17	1	1	.	3	5

This correction affects neither the correlation nor the Figure on page 287 since both derived from the correct data.

PUBLICATIONS RECENTLY RECEIVED

The Organization of the Cerebral Cortex. By D. A. SHOLL. London: Methuen. 1956. Pp. xvi+125. 18s.

This admirable little book by the Reader in Anatomy at University College, London, should find a place in every psychological library. Indeed, I would make it obligatory reading for every student of psychology proposing to embark on the fashionable grand tour through the Hull and Hebb country.

After a brief account of the available methods of histological study, Dr Sholl proceeds to outline the earlier work on the structure of the cortex. The outcome is devastating to anyone brought up in the tradition of Brodmann, Campbell and the Vogts. On cortical stratification, for instance, Dr Sholl writes: 'It is clear that each observer has his own qualitative and subjective criteria for his schema of lamination and there is no agreement as to the definition to be adopted for a discrete cortical lamina' (p. 19). Cytoarchitectonic maps fare little better. Although it can be accepted that certain regions of the cortex show differences in histological structure, '...attempts to subdivide these main regions on the basis of criteria subjectively applied to preparations stained by a selective technique are unsuccessful' (p. 28). This conclusion may well cause many of us to regard the standard texts on the physiology of the brain with a wavier eye. Indeed, Dr Sholl comments that the conventional criteria of structural differentiation, taken in isolation from any consideration of connectivity, are unlikely to be correlated at all closely with the essential activity of the central nervous system.

Dr Sholl next considers the quantitative approach to the histology of the cortex—an approach largely due to Bok and his followers and to which the author has himself made important contributions. This approach deals with such attributes of structure as the total thickness and extent of the cortex, the total number of neurons and the density of neuronal packing, the number and relative thickness of the laminations, and the proportions of specific types of neurons in the different cortical areas. It is clear that a good many facts about these and related problems are now known at the gross statistical level, but technical difficulties remain very great. From the point of view of an understanding of function, the outcome of quantitative neurohistology is not as yet very impressive.

Following a most useful discussion of neural connectivity, Dr Sholl examines briefly some aspects of cortical organization as revealed by neurophysiological and clinical inquiry. This part of the book is perhaps less satisfactory than the rest, largely owing to limited coverage and drastic compression. It is concluded that whereas parts of the cortex, owing to their two-way connexions with subcortical structures may well be associated with specific types of behaviour, '...the main volume of cortex cannot be described in terms of specific "functions"' (p. 80). One may point out that this conclusion, though doubtless acceptable to Lashley, is perhaps misleading unless appropriately qualified. In so far as clinical material is concerned, it is noteworthy that the author limits himself almost entirely to operations on the frontal lobes, the effects of which are notoriously difficult to evaluate. Had he considered comparable data relating to the parietal or temporal lobes, the impression given might well have been very different. True, nothing is harder than to define a cortical 'function' by inference from the derangements produced by cortical lesions. But unless the effort is made it is doubtful whether cerebral neurology can advance further.

In his later chapters Dr Sholl surveys briefly the mode of operation of the cortex in the light of some current theories. The Pavlov theory is dismissed as totally inadequate. The theories of Lashley and Hebb are regarded more favourably, though certain difficulties of both are examined. Hebb, in particular, is taken to task for his undue reliance on the data furnished by strychnine neuronography and, more generally, for failure to provide a quantitative application of his theory. (No doubt Dr Hebb would be the first to agree with these criticisms.) It is, however, pleasing to note that Hebb's book is considered as '...possibly the only one that presents psychological problems in a way that can be appreciated by the anatomist and physiologist' (p. 91).

Cybernetic theories likewise come under fire. On Ashby: 'It is true that examples of adaptive behaviour in animals are given that do not conflict with the notion of ultrastability, but there is no evidence that in fact, the central nervous system does embody this principle and no indication is given as to what anatomical and physiological facts correspond with the design' (p. 93). On Grey Walter: 'It is most unlikely that the unique complicated circuits suggested by the author's diagrams exist in any nervous

system' (p. 94). Biologists, Dr Sholl urges, are interested not only in what an animal does but in how it does it. Psychologists attracted by electronic models could do worse than ponder his words.

Dr Sholl's positive contributions to theory are modest but far from trivial. In view of his interest in the quantitative and statistical approach to cerebral structure, he not unnaturally leans towards a quantitative and statistical treatment of cerebral function. He is much impressed by recent developments in mathematics and their application to biological problems. He sees hope, for instance, in the sort of ideas recently put forward by Beurle and by Uttley, though is modest enough to state that a theory recently advanced by Uttley and himself '... seems to demand a cortical organization for which there is no adequate evidence' (p. 105). Although not everyone will agree that the theory of games offers the best basis for an understanding of behaviour, it is to be most earnestly hoped that Dr Sholl and his associates will carry their ideas as far as they will possibly go.

This little book is 'the genuine article'. It is expert and informed, restrained yet adventurous. It would prove an astringent corrective to much nonsense talked about brain function by those who should know better. Psychologists—who don't always know better—should read the book with particular care. What if Pavlov does turn in his grave? Dr Sholl is only putting into practice Pavlov's own dictum that in the study of the nervous system every step of thought should be verified through agreement with actual fact. And if neurons are not facts, what are?

O. L. ZANGWILL

Earning and Instinct in Animals. By W. H. THORPE. London: Methuen. 1956. Pp. viii + 493. 55s.

Dr Thorpe is exceptionally well qualified for writing a non-partisan text on animal behaviour. His extensive knowledge of ethological, psychological and naturalistic source materials enables him to put together and often to integrate a great diversity of facts and theories. The result is a book that will orient any reader and provide him with a wealth of new information.

The book has three parts. The first contains an examination of the concepts of 'directiveness' and 'purposiveness', and a critique of instinct theory. The point of view adopted is purposive and organizational. Cognitive processes are emphasized and it becomes clear that the author finds it congenial to use terminology of expectancies, hypotheses and the like. A general drive to explore and perceive the environment is postulated. Perceptions may be goals. Drives may be endogenous in the sense that they arise with the C.N.S. itself. The review of instinct theory justifies the acceptance, with some reservations, of Lorenz's notion of action specific energy. Tinbergen's hierarchical schema of energy channels is criticized because of its rigidity and, to account for the 'flexible directiveness of appetitive behaviour', Dr Thorpe finds it necessary to postulate some process of working towards 'ideal' or 'full' releasers.

In Part II some of the features of habituation, conditioning, trial and error learning, latent learning and insightful learning are examined. Imprinting is not given the status of a distinct form of learning. The discussion of habituation is particularly valuable since this topic has not received from psychologists the attention it deserves. Latent learning is found to involve a simultaneous reorganization and integration of past experiences and thus to some extent to be insightful. The experiments of the rat laboratory gain significance by being discussed in the general context of orientation behaviour as it occurs in a variety of animal forms, invertebrate as well as vertebrate. A final chapter deals with some recent speculations on the neurophysiological mechanisms of learning.

Part III, more than half of the book, is headed 'The learning abilities of the main animal groups', but the range is wider than this may indicate. Birds and arthropods receive the fullest treatment in chapters that are extensions and revisions of previously published journal articles. The new material includes some interesting studies of the solution of string-pulling problems by birds. It is perhaps a little disappointing to find only 16 pages allotted to protozoa, coelenterates and echinoderms. The chapter on mammals is also short, but justifiably so, and the principle is adopted that mammalian behaviour should be regarded as learned unless proved otherwise. Throughout this part of the book experiments have been selected for mention because of their relevance to the biology and natural history of the animals concerned. This ensures that learning is not treated as an isolated process. The classification adopted earlier in the book proves to be convenient, whatever other justification it may have.

Dr Thorpe's final comments concern the unity of behaviour as it appears in diverse animal forms and the unity of the physical and biological sciences.

Much of the book consists of short descriptions of experimental work coupled with shrewd interpretative comments. Theories are dealt with in critique rather than in expository form. The level of difficulty

makes the book unsuitable for the beginner but excellent as an advanced text or reference. In one place or another, attitudes are expressed towards all the main problems of behaviour. While a consistent perceptual-organizational point of view is maintained, there is no attempt at formal theory construction. On occasion the absence of Occam's razor is noticeable, but not to be regretted in this case since a more constrictive approach would have reduced the scope of the book and deprived the reader of some valuable insights.

The merits of the book as a whole are so outstanding that criticism tends to be inhibited. However, there are three sources for dissatisfaction. First, terms like 'instinct', 'purpose' and 'drive' are not used consistently. Secondly, the reader too frequently meets assertions that cannot be interpreted with any precision even in their context, for example, '... perception is a basic characteristic of the drive of the living animal'. Thirdly, the point of certain theories seems to have been missed and in places unconsidered criticisms are made. For example (p. 103), we are told that the only possible interpretation of avoidance training is that an 'anticipation' (author's quotes) of the noxious stimulus has been acquired, and that to postulate 'fear' as a drive is a superficial stratagem because the term connotes anticipation (without quotes) by definition unless the James-Lange theory is accepted.

The publication of this book is an important and timely event for all students of animal behaviour. It is to be hoped that it finds the place it deserves on the bookshelves of psychologists and ethologists alike, and that its integrative approach and its provision of common background information will help to promote a more unified outlook.

R. L. REID

Analysis of Perception. By J. R. SMYTHIES. London: Routledge and Kegan Paul. 1956. Pp. xiii + 140. 21s.

The most remarkable feature of this very interesting book is that it combines two approaches, the neurological and the philosophical, which are usually kept separate. The author is a neurologist who has made a careful and detailed study of recent philosophical literature. As a philosopher he is able to clarify the important but obscure concept of 'the body-image': and as a neurologist he is able to point out the weaknesses of current philosophical theories of perception. He is also an expert on hallucinations, especially those induced by mescaline, and his chapter on 'Veridical and Hallucinatory Sense-experiences' is one of the best in the book. His main aim, however, is to show that 'a synthesis of the sense-datum theory with the neurological account of perception may constitute, if its basic terms can receive adequate definition, the only valid theory of perception in both science and philosophy' (p. 4).

He defines the term 'sense-datum' itself by reference to after-sensations: roughly, a sense-datum is an entity such that it or some part of it can have a common boundary with an after-sensation. (The term 'after-sensation', he says, needs no formal definition; it can be defined operationally and ostensively.) Sense-data are spatial entities; they can even be measured, e.g. by reference to the total diameter of the visual field. In these respects, there is no difference between hallucinatory sense-data and normal ones. Here, and throughout, Dr Smythies is mainly concerned with *visual* sense-data, though he does have something to say about somatic sense-data as well. His version of the sense-datum theory could no doubt be applied to the other sense-modalities, but some additional definitions would obviously be needed. He then argues that the Naive Realist theory, which tries to dispense with sense-data altogether, is inconsistent with the empirical facts about the finite velocity of light. The theory of 'psycho-neural identity', which identifies sense-data with physiological events in the brain, is rejected on the ground that the relevant cortical events are not geometrically congruent with the sense-data alleged to be identical with them, while the obscure doctrine of the 'projection of sensations' only makes confusion worse confounded. Dr Smythies therefore accepts a dualistic interaction theory. But it differs from the traditional Cartesian theory in two important respects. According to Dr Smythies, the mind is itself an *extended* entity, because the extended sense-data which it senses are actually constituents of it. On the other hand, physical space is not the only space there is. There are also a number of sensible spaces, a different one for each percipient in which sense-data are located. Thus we have a choice between two unorthodox cosmological theories: in the first, there are no spatial relations between sense-fields and physical objects, and then we have a very complicated universe in which there are many different spaces; in the second there is an all-embracing n -dimensional space, of which physical space and the various sensible spaces are three-dimensional sections (where n , if I understand rightly, = 6). Dr Smythies, I think, is inclined to prefer the n -dimensional theory; it would follow that psycho-physical interaction must occur across a 'dimensional interface'. He also suggests, however, that at some future time we may be able to devise experiments which will make it possible to decide between the two theories on empirical grounds.

Dr Smythies frankly calls his theory of perception a representative theory. The great difficulty of representative theories, from Locke's time until now, is that they seem to make it impossible to us to know anything about the physical world. Dr Smythies tries to meet this by drawing two closely connected distinctions: (1) between sensing and perceiving, (2) between direct and indirect observation. He holds that we do really perceive the physical objects we suppose ourselves to perceive: the sensing of a sense-datum is only a part of the total process of perceiving, not the whole of it. Again, sense-data are indeed the only entities we observe *directly*; but in so doing, we *indirectly* observe the physical objects which they represent.

But if sensing is only one part of the total process of perceiving, what is the other part? The other part consists in the operation of a very complex 'non-symbolic representative mechanism'. This ensures that the spatial properties of sense-data will correspond pretty closely with the spatial properties of the physical objects which set the mechanism going by stimulating the sense-organs. Dr Smythies claims that we can get light on the nature of this mechanism by studying stroboscopic phenomena, which suggest strongly that it works on much the same principles as a television apparatus (ch. 2). Because our nervous system does contain a pretty reliable representative mechanism of this kind, we are justified in maintaining that by direct observation of sense-data, we are indirectly observing physical objects.

However interesting and suggestive this may be from a causal point of view, I cannot think it gets over the epistemological difficulty. Dr Smythies, like Locke before him, does not seem to have distinguished sufficiently clearly between two different situations: (1) x senses a sense-datum s which is *in fact* produced by a representative mechanism affected by the object O , and the spatial properties of x do in fact correspond with the spatial properties of O . This might be called the *de facto* sense of the word 'perceive'. (2) When x senses a sense-datum s produced in this way, he is thereby enabled to *discover* or *find out*, non-inferentially, that O exists and has these spatial properties. This might be called the cognitive sense of the word 'perceive'. However true the propositions listed under (1) may be, we need to establish the truth of (2) as well, if the classical difficulties of the representative theory of perception are to be solved.

H. H. PRICE

Fundamental Statistics in Psychology and Education. By J. P. GUILFORD. New York and London: McGraw-Hill. Third edition, 1956. Pp. xi+565. 47s.

The Measurement of Abilities. By P. E. VERNON. London: University of London Press Ltd. Second edition, 1956. Pp. xii+276. 20s.

Although very different in scope these two new editions may conveniently be noticed together, in view of similarities both in subject-matter and in the principles underlying the revision. Each is quite clearly the same work as before, but brought up to date by the inclusion of a small proportion of fresh material. In each case, too, the text has been entirely reset, with more printed matter to the page, and consequently a smaller total number of pages.

The difference in scope probably explains why, at first sight at least, there appear to be greater changes in Guilford's book after six years than in Vernon's after sixteen. Recent refinements in statistical method and changes in emphasis have allowed Guilford to discard some things that appeared in earlier editions: thus Probable Error—which Vernon continues to treat fairly fully—is dismissed in a very brief footnote and not even defined. The disappearance of certain other material, notably the chapter on scaling procedures, is explained by its having been transferred to the author's *Psychometric Methods* (review in this *Journal*, 1955, 46, 145-6). (Similar transfers having taken place in the reverse direction, successive editions of the two works would seem to be progressing—out of phase, so to speak—along parallel paths.) Another relatively major change is the expansion of the previous single chapter on Testing Hypotheses to two. Guilford comments on the progress that has been made in this area, but maintains his bias towards descriptive rather than sampling statistics, on the grounds that 'research in the social sciences not confined to studies in which results are at the margin of significance', and that 'the generation of scientific ideas... does not depend particularly upon decisions concerning change alternatives'. There are several useful new tables, including one for reading off cosine-pi correlation coefficients from values of $\sqrt{bc}$.

Modifications in the statistical chapters in Vernon's book are, necessarily, less extensive, but it is worth noting that such topics as chi-squared (and degrees of freedom generally) and small-sample statistics are dealt with more adequately than was possible at the time the book first appeared. It is in the sphere

of the theory and practice of intelligence testing, particularly in relation to current problems in educational administration and research, that the more important changes have been made. The work thus retains its unique character as a comprehensive survey of all aspects of testing and measurement in education, including traditional examinations, and its genuine comprehensibility to the non-mathematician has not been attained at the cost of misleading over-simplification. The invaluable classified list of tests available in this country, now completely reconstructed, is exemplary for the clarity of its layout.

BORIS SEMEONOFF

Discussions on Child Development. Vol. I. edited by J. M. TANNER and B. INHELDER.
London: Tavistock. 1956. Pp. 240. 25s.

This praiseworthy volume augurs well for the series of which it is the first. In 1953 the World Health Organization brought together a number of distinguished scholars to discuss, in the words of the book's subtitle, 'the biological, psychological, and cultural approaches to the understanding of human development and behaviour'. This study group contained seventeen members and represented the fields of psychology, psychiatry, ethology, cultural anthropology, electro-physiology, and psychoanalysis: it included, to name but a few, John Bowlby, Bärbel Inhelder, Konrad Lorenz, Margaret Mead, Jean Piaget and Grey Walter. This book gives a running account of the group's discussions and the editor must be congratulated on having 'tidied up' the transcript without depriving it of its friendliness and informality. The structuring of the discussion itself represents a worthwhile experiment in inter-disciplinary communication. To begin with, each participant introduces himself by giving a brief autobiography and a sketch of his special interests. Then follow eight sessions, each of which is opened by a paper from one member on some aspect of development. The ensuing discussions are the important thing and, as might be expected from the calibre of the participants, they are erudite, wide-ranging, and stimulating: in them, the same or analogous phenomena are looked at from varying viewpoints, and there emerge a host of interesting facts and exciting ideas. It would be impossible to select any one portion of the discussions for particular mention: suffice it to say that there was hardly a page which did not provide the reviewer with some unthought of parallel or some new idea. The aim of the meeting was not to read papers, pass resolutions, or issue reports but to discuss human behaviour in a permissive and multi-disciplinary atmosphere and achieve, thereby, some measure of understanding and cross-fertilization between the several disciplines which converge to study behaviour. For once, a meeting of this kind can fairly be said to have achieved its aim and the book's many readers will be grateful to assume the role of eaves-dropper. The reviewer can think of no psychologist, however specialized his interests, who will not derive both pleasure and profit from this volume.

I. M. L. HUNTER

Alfred Adler: an Introduction to his Psychology. By LEWIS WAY. Harmondsworth:
Penguin. 1956. Pp. 252. 3s. 6d.

This latest addition to the Pelican Psychology Series contains a brief biographical sketch of Adler, and an outline of his theories, illustrated by many extracts from his writings. It will be interesting and useful both to the student and the general reader. Though he was a first-rate speaker, Adler was a diffuse and repetitive writer, and he is an excellent subject for the boiling-down process to which he is here subjected.

The book should dispose finally of certain misconceptions about Adler, as that he was a Nietzschean power-worshipper who regarded aggression as the dominant human motive. 'The striving of every living individual', he wrote, 'is directed towards achievement, not towards power'. *Gemeinschaftsgefühl* or 'social interest' is one of the basic Adlerian concepts; and the neurotic, in Adler's view, is essentially a person who is unable to achieve satisfactory relations with others owing to the self-absorption and touchiness produced by inferiority-feeling.

Adler called his system *Individualpsychologie* to express his view that the person must be considered as an individual whole—that symptoms cannot be treated in isolation, but must always be seen in relation to the total personality and to the style of life. He was not an individual psychologist in the current sense; in fact, he was a pioneer of social psychology. In his first published work, written when he was 28 he said: 'The doctor can no longer ignore the standpoint which examines men, not as isolated units, but as products of their society.' Later, he was one of the first to point out the importance of role and culture pattern in the formation of personality. Like most psychologists of his day, he was apt to make sweeping

generalizations, prefaced by some such phrase as 'I have observed' or 'it is evident'. But he had a remarkable knack of hitting the nail on the head, and many of his pronouncements have been amply confirmed by later investigation. To take only one example, his statements about the effects of deprivation of affection in early childhood (cf. pp. 101-2) provide a striking anticipation of the work of Bowlby.

MARGARET KNIGHT

Introduction to Psychology. By CLIFFORD T. MORGAN. New York and London: McGraw-Hill, 1956. Pp. vii + 676. 45s.

The choice of an introductory text for students of general psychology is influenced by many considerations, the least mundane being the chooser's theory of education. There is much to be said for each of the possible approaches—the historical, the biological, the methodological, the broad survey of the whole field, and so on. This book is of special interest since Prof. Morgan has recently devoted his time to introductory teaching. His conclusion seems to be that a text should 'give them the facts'. In this remarkably comprehensive survey of the contemporary scene controversial issues are avowedly omitted. There are four pages on historical background, including the 'schools', and six on methodology (as opposed to techniques), but in the rest of the book there is little or no attempt to relate findings critically to past theory or present method. Although occasionally the reader is told that one experiment is good or better than another, he has to take the author's word for it.

The book has been arranged so that only selected chapters need be read depending on which of three courses the student is following a 'life oriented' course, a 'science oriented' course, or a comprehensive course. In fact the whole book is life oriented in that findings are continually related to everyday experience. Various chapters have been drafted by fourteen psychologists, but the main author has rewritten them all to avoid stylistic changes. The result is extremely readable, and, as far as this reviewer knows, accurate. The book would be appreciated by the readers of a good popular science periodical. The sections on physiology are somewhat more learned, which might be expected under this author. The lists of other reading at the end of each chapter are really excellent. About one quarter of the space is taken up by pictures and tables, many of them gorgeous, and many unnecessary. There is a glossary which contains 'apes' and 'brain' but not, for instance 'ambivert', 'catharsis' 'cerebellum' and 'central tendency', which are all used in the book.

Those psychologists who take psychology to be a suitable subject for a liberal education, in that it provides exercise in clear thinking and stimulates intellectual excitement, may not be enamoured of this text. However, those who wish to prescribe a simple, clear, up-to-date and thoroughly comprehensive summary of present-day psychology will find it at least the equal of several other popular introductory texts, and more recent than most. The chapters comprising the 'science oriented course' might prove particularly suitable for those reading psychology as part of a general degree.

B. M. FOSS

Rank Correlation Methods. By M. G. KENDALL. London: Griffin. Second edition, 1955. Pp. vii + 196. 40s.

This is a welcome re-issue, with much new material, of Prof. Kendall's important work, first published in 1948 and apparently little known to the majority of psychologists, although its influence on design and treatment in recent published experiments has been considerable.

Although the scope of the book is comprehensive, the main content is an exposition of the theory and use of the rank correlation coefficient τ (tau), which has the advantage over ρ that its significance can be estimated with much greater accuracy. It is, however, more cumbersome to calculate, since it involves comparing ranks for all possible pairs of objects or individuals. This number of operations, i.e. $\frac{1}{2}n(n-1)$, quickly rises to formidable proportions for any but a very small sample, and indeed certain of the appendix tables, as well as the demonstrations in the text, seem to imply that samples of 10, or at the very most 12, will be the largest requiring to be handled. Further, obtained values of τ are always less than those of ρ and it is consequently of less value as an estimate of product-moment correlation.

An additional merit of τ , however, is its generality, i.e. it can be extended to a wide variety of purposes, such as biserial and partial correlation and paired comparisons. While recognizing that 'a tool which has many purposes is not usually as efficient for any one of them as a specialized tool developed solely for a single purpose', the author sums up by pointing out that 'there are situations where the power of rank-

order tests is surprisingly high and little seems to be lost by employing ranking methods'. There are undoubtedly circumstances, particularly among those involving evaluative judgments, in which Prof. Kendall's methods are the most appropriate of those available, and no statistically-minded psychologist can afford to neglect to add them to his repertoire. Since most of even the new and recently revised statistical texts are curiously reticent on the subject, the present book is indispensable.

BORIS SEMEONOFF

Opinions and Personality. By M. BREWSTER SMITH, JEROME S. BRUNER and ROBERT W. WHITE. London: Chapman and Hall. 1956. Pp. vii+294. 48s.

In 1947 the authors of this book, three American Social psychologists, induced ten men from different walks of life—a law clerk, a salesman, a factory operative, a real estate agent, and so on—to submit for two hours a week for fifteen weeks to a battery of tests and interviews. The object was to find out as much as possible, from their twenty-eight 'procedures', about the personalities of their subjects, and from this to judge the way in which their opinions on a particular topic fit into the personality pattern which the investigators had constructed for each of them. The topic chosen was 'Russia', a topic on which everyone may be expected to hold some opinion, and one about which many people feel strongly. All the subjects except one were hostile in varying degrees; the exception, while not being a member of the Communist Party, was strongly in favour of the Soviet regime. In passing it may be said that it was unfortunate that the investigators did not manage to find more people in this position.

The project was intended to be something of a corrective to the simpler statistical approach. In the view of the writers, the correlation of a single trait or constellation of traits with opinions 'pro' or 'con' is all too simple. The object towards which the attitude is felt is itself complex. In this case 'the Russia to which each referred was a conception selectively fashioned, a reflexion of individuality'. Moreover, a hostile or favourable attitude may function in one way for one person and in another way for another.

There are, it is suggested, three main functions served by opinions and attitudes. First, 'each person is constantly trying to size up the world around him and to place it in relation to his major interests'. Attitudes, in fashionable jargon, are 'structuring' agencies. Secondly, a man is influenced by such need as he may have to conform, or deliberately not to conform. Thirdly, attitudes may be vehicles for the expression of inner problems. The upshot is that an opinion may be considered as 'a resultant or compromise between reality demands, social demands and inner psychological demands'. Furthermore, they are influenced by the availability of information, by intellectual ability, and by the holder's 'expressive style'—whether, for instance, he is a man of theory or a man of action.

All this is worked out in detail for three of the subjects, and in less detail for the other seven. The case for multiple causality is, as we should expect, made out. The case histories, indeed, are models of exposition. The results, as I have suggested, are not surprising, but it is well that we should be reminded of a complexity all too often forgotten. Perhaps the most significant aspect of the results is the implications they carry with respect to attitude change. If, for example, a man holds certain views about Russia simply because he is interested in the subject, then his views may be changed by further information. If, on the other hand, his views serve to make him feel he is in the right company, or if he is expressing some inner tension, then information will be of no use, and other methods must be used. Propagandists must take into account the variety of vested interests which may lend support to any opinion they want to alter.

W. J. H. SPROTT

The Psychology of Industrial Conflict. By ROSS STAGNER. New York: Wiley. London: Chapman and Hall. 1956. Pp. ix+550. 64s.

Human Relations in the Industrial Southeast. By GLENN GILMAN. North Carolina University Press. London: Cumberlege. Pp. xii+327. 40s.

Textbooks and case-studies of 'human relations in industry' continue to pour from the presses. It is good to see that the latest volumes represent a distinct improvement on the earlier ones—the ritual chant 'workers are human beings!' which formed much of the substance of the early message of the human-relations movement has now given place to a sober examination of institutional demands upon the individual (including the influences of unions and employers' associations), the part played by historical factors in industrial conflict, and the diverse motivational patterns of occupational groups. There has

been a refreshing tendency for industrial research to be brought within the ambit of sociological and psychological theory. Dr Gilman, for example, offers the reader an extended industrial case-study, presented in sociological terms, while Prof. Stagner attempts the first comprehensive and systematic psychological approach to industrial conflict.

As a text-book, *The Psychology of Industrial Conflict* is first-rate. The book is well laid out, so that one can move from one chapter to the next by a systematic line of thought. It opens with a survey of research on perception and motivation, first in their general aspects (with the emphasis on social differences) and then in their application to industrial life. This is followed by chapters on frustration and aggression, social groups, institutions and leadership, in each of which the relevant industrial material is presented within a framework of concepts which relates it (somewhat eclectically, it is true) to research work going on outside the industrial field. Later chapters take up the industrial theme more directly: management and union tactics, the strike, and industrial co-operation. The coverage of published research is remarkably complete, including British as well as American work up to 1954. The result is a rather fat text-book of over five hundred pages, which can be taken, by those who like an approach on a broad front, in conjunction with Kornhauser's two volumes to provide a comprehensive introduction to an important group of problems.

Dr Gilman is a Chicago-trained sociologist specializing in industrial management at the Georgia Institute of Technology. His book is in part a case-study, describing the growth of the textile firms located in south-eastern U.S.A., in the southern Piedmont region. If he had gone no further he would have produced an interesting historical study of an important American industry, limited in appeal to those who are concerned with textiles. But Dr Gilman has chosen to present his material 'against a frame of reference that is sufficiently fundamental to serve the same purpose for many industries in many regions'. The frame of reference is provided by the 'work, place, people' concept of Geddes, derived from Play. This is developed into an attempt to interpret the history of the Piedmont textile industry as an expression of basic human nature (compound of 'needs for understanding', security, and novelty), utilizing available natural resources and available cultural resources. Inevitably, since this is primarily a sociological study, the psychological treatment is rather rough-hewn. On the cultural level, a useful working distinction is made, between a folk-society and a mass-society (substantially the distinction made by Simmel between *Gemeinschaft* and *Gesellschaft*). Eighteen differences between the two social types are tabulated in a check-list, and the Piedmont region is seen to be a folk society, emphasizing solidarity, to use rather than specific ties between individuals, and homogeneity rather than heterogeneity. The industry in such a society takes on a characteristic form, differing in many ways from industry of the mass type in a mass-society. This long and very detailed case-study forms a useful addition to research on the 'community factor in modern technology'. At the end, the author has included a couple of dozen photographs of modern textile plants and their surroundings, showing the physical appearance of mill communities.

J. F. MORRIS

Follow-up Study of War Neuroses. By N. Q. BRILL and G. W. BEEBE. Washington: Veterans Administration. 1955. Pp. xviii + 393. No price given.

The authors report a follow-up study, conducted under the auspices of the Veteran's Administration, of a sample of 1475 white men (excluding officers) of the U.S. armed forces who had missed duty during 1944 on a diagnosis of psychoneurosis. Of these cases, about one-third were studied from records only, while about two-thirds were interviewed by a psychiatrist approximately five years after their breakdown in 1944. Two hundred and twenty-five psychiatrists took part. Information about two control groups was obtained, one of enlisted men in 1944, the other of enlisted men in 1951. A large number of factors, connected with both service experience and pre-service experience and personality, are considered in relation to the breakdown and to mental health at the time of follow-up. The findings are set out in a very large number of tables. Many of the assessments must be regarded as rough and tentative, but in the main the results are more or less what one would expect. An interesting point is that, at the time of follow-up, about 60% of the men felt that they still needed treatment. But 'there is no evidence that treatment played an important role in the general improvement which occurred between separation and follow-up'. In the concluding pages, the authors express the view that 'predisposition has been overgeneralized, and we would do well to think more in terms of vulnerability to specific forms of stress and to the factors which may modify that vulnerability', that is, attention should be directed to the effective use of those neurotic tendencies, and not merely to trying to keep them as far as possible out of the forces.

D. GRAHAM

Self-evaluation and Rejection in Groups. By JOACHIM ISRAEL: Uppsala: Almqvist and Wiksell. 1956. Pp. 250. Sw. kr. 20.

This monograph consists of a review of the literature, a 'conceptual outline', and reports of three laboratory experiments. It is very much in the Michigan tradition and draws heavily on the experiments and theories of Leon Festinger and his group. The first experiment is the Swedish part of the cross-cultural study of rejection of deviates, directed by Schachter. The results show insignificant tendencies in the predicted direction, though the deviates *were* rejected here. The second experiment tests some of the author's hypotheses concerning rejection of deviates of different ability in co-operative and competitive groups, and these are largely confirmed. The third experiment shows some of the conditions under which the group's evaluation is accepted as opposed to a conflicting self-evaluation. All three experiments were skilfully conducted and carefully interpreted though the write-up is very hard to follow.

The review of the literature and 'conceptual outline' are unusual in the amount of discussion given to questions of definition, not all of which is illuminating. A series of hypotheses is developed, but these are not deductively related as in Festinger's theory on which they are based: each is asserted independently, following a short discussion of the problem.

MICHAEL ARGYLE

Performance Tests of Intelligence under Indian Conditions. By C. M. BHATIA. London: Oxford University Press. 1955. Pp. xi + 131. 17s. 6d.

This short book, with a foreword by Sir Godfrey Thomson, is a straightforward account of the construction and standardization of a battery of individual performance tests suitable for use with Indian children. The battery consists of part of the familiar Kohs Block Designs and Alexander's Passalong Test, together with tests of Pattern Drawing, Immediate Memory and Picture Construction, devised by the author.

The battery has been administered by Dr Bhatia and his co-workers to representative samples of literate and illiterate Indian children ranging from 10 to 16 years of age and a separate conversion table drawn up for each of the two groups of children. There is an interesting, though brief, account of the special measures taken by the testers to establish good relations in the villages visited, a prime necessity in obtaining a suitable atmosphere for testing, much of which seems to have been carried out in the open with an audience. A more detailed account of the difficulties met with in testing might have helped to augment the Western reader's insight into the Indian scene.

Not unexpectedly, the mean performance of literates is found to be superior to that of illiterates on every test. The author maintains that illiteracy in India is related, not to differences of race or of culture but to lack of educational opportunity. His two independent standardizations for the two groups reflect his insistence that valid comparisons are restricted to subjects within each group separately, and that no such inter-subject comparisons should be made across the groups. The chapter on 'The Nature of Intelligence' is sketchy and might well be expanded.

A. E. G. PILLING

Allgemeine Theorie der menschlichen Haltung und Bewegung. By F. J. J. BUYTENDIJK. Berlin: Springer. 1956. Pp. 367. D.M. 39-60.

This is a German translation, and therefore a more generally accessible version, of the author's *Algemene Theorie der menselijke Houding en Beweging*, published in the Netherlands in 1948. It considers human posture and movement from a point of view intended to collate physiological and psychological data. Human bodily activities, must, it is claimed, be regarded as 'functions', which differ from 'processes' in being 'indivisible whole(s), consisting of changes which are meaningfully related to something outside themselves'. The something outside may be, for example, a goal to which an activity is directed and which gives it its value. Furthermore, human behaviour must be seen as the behaviour of a *self*, which is neither solely physical nor solely mental but partakes of both realms in a psycho-physical unity. Not only associationism and the Pavlovian type of reflex theory but also Gestalt psychology, with which Buytendijk's position has much in common, are blamed for leaving no room for the 'Subject', which 'disposes of its own body', 'manifests itself as the cause of the perceived movement' but is not localizable in the nervous system.

The questions most contemporary psychologists would like to ask about human posture and movement—in what conditions are particular sorts of movement most likely to be performed? How can movements be predicted and controlled? What is the precise nature of the neuromuscular mechanisms underlying movement?—receive comparatively little attention, apart from assorted data mentioned as evidence for the main theses of the book. There is, in fact, little of what most of the readers of this *Journal* think of as psychology. On the other hand, there is a great deal of phenomenology, the method of inquiry introduced by Husserl and extended by many continental philosophers, notably the existentialists, which leads to various conclusions from the fact that we cannot help thinking about things in certain ways.

It is interesting to ask what impression of psychology the reader of the book will obtain. One fact that will not spring at him is that most of the psychology in the world, for good or for ill, has for some time been the product of English-speaking countries. Nor will he see that recent years have been at least as prolific as any others of innovative developments. Remarkably few of Buytendijk's references are later than 1939, and almost all of those that are come from continental Europe. The reader will not be given the impression that there are many contemporary theories in psychology which agree with Buytendijk that behaviour does not depend solely on external stimuli of the moment, that it is often purposeful and that various pieces of behaviour and neural processes interact, and yet do not accept Buytendijk's somewhat vitalistic conclusions. Finally, it must be admitted that Buytendijk disdains any meretricious devices that might encourage people to think that psychology is all froth and glitter. The text is written in a somewhat viscous style and is unrelieved by any illustrations, tables or graphs. It is hard to believe that anybody whose staple diet is neither German philosophical literature nor suet pudding will find the book an exhilarating one to read.

D. E. BERLYNE

A New Psychotherapy in Schizophrenia. By M. SECHEHAYE. New York and London: Grune and Stratton. 1956. Pp. vii + 199. \$4.50 net.

In two previous publications (*Symbolic Realisation* and *The Autobiography of a Schizophrenic Girl*) Mme Secheyaye described her highly original and successful treatment of a young schizophrenic girl by a psychotherapeutic method which was called 'Symbolic Realisation'. The present publication is essentially her attempt to order her previous observations into a systematic theory which will allow others to apply her method in treating schizophrenic patients.

Although the author offers a completely psychological mode of treatment she views schizophrenia as the result of the impact of early trauma on a constitutionally predisposed schizoid individual. She thus accepts the concept of a 'latent schizophrenia' which may become actualized by the frustration of fundamental needs, tolerated by the non-predisposed individual. The method of symbolic realization sets the therapist the task of satisfying these unrelieved needs at a symbolic level appropriate to the genetically primitive ego state of the patient and thus 'deliver him from the awful guilt holding him captive and driving him to his death'. The author's obvious empathy with the schizophrenic patient allows the reader to accept the frequently flamboyant style of expression which might otherwise have been objectionable. The vivid account of her patient's therapeutic progress leaves one in no doubt as to Mme Secheyaye's ability to enter the schizophrenic's 'world of experience' (the existential *Erlebniswelt*). It is, however, difficult to assess the validity of her case for 'symbolic realization' as a curative agent. Its essentially subjective and often obscure nature suffers from the added difficulty that Mme Secheyaye relies primarily on retrospective reports of the now non-psychotic patient to verify the validity of her early interpretations of the patient's psychotic behaviour. An even more fundamental weakness lies in the fact that the method and underlying theory is based solely on the author's treatment of one schizophrenic patient and, as the author admits, 'it remains to be seen whether utilizing the same method in the same way, under conditions as alike as possible, and with similar symptoms, the same phenomena of cure will appear again'.

However, the value of this book cannot be judged by the validity and applicability of the author's therapeutic method. In an attempt to understand the subjective world of the schizophrenic Mme Secheyaye utilizes concepts from the diverse fields of psychoanalysis, existential phenomenology and the genetic psychology of Piaget. It is impossible to mention all the stimulating ideas raised by the author who has obviously a deep and sympathetic understanding of the schizophrenic patient. Although most of her information is gathered from her experiences with one patient, there is much to be said for the assertion of Goldstein and others that the intensive and detailed study of one patient is often of more

significance than a more superficial study of many. Whether or not we are impressed by Mme Sechehaye's therapeutic method, no one who professes an interest in the prime clinical problem of schizophrenia can afford to neglect consideration of the author's absorbing observations.

A. MCGHIE

Les Mécanismes Cérébraux de la Prise de Conscience. By PAUL CHAUCHARD: Paris: Masson. 1956. Pp. 240. 1300 frs.

Dr Chauchard attempts to achieve a biological conception of consciousness based largely on neurophysiological considerations. His masters are Lapique and Pavlov, but he is conversant with a good deal of the relevant data from clinical neurology and psychiatry. He is antagonistic to both classical dualism and the new materialism of Cybernetics, seeking an understanding of consciousness in terms of the evolution of the central nervous system itself. So far as the present reviewer can judge, nothing particularly original emerges from his study. None the less, it can be commended as an essay of good standing in the French physiological tradition.

O. L. ZANGWILL

Emotional Hazards in Animals and Man. By HOWARD S. LIDDELL. Springfield: Thomas. Oxford: Blackwell. 1956. Pp. xi+97. 20s.

Dr Liddell has been studying abnormal behaviour in animals for some thirty years, and this little book, based on a series of lectures given to a psychiatric audience in Halifax, Nova Scotia, represents the outcome of his experience. After a vivid description of a demonstration of conditioned foreleg flexion by a sheep, a chapter on the reassuring effects of the presence of the mother during the conditioning of young animals follows. Liddell then discusses those aspects of the conditioning procedure which he considers the most stressful for animal subjects, and which he terms loneliness, monotony, confusion and overstimulation. The manifestations of 'neurotic' behaviour in his experimental animals are then described, to which some notes by Mrs Prochazka on the effects of faulty training procedures in horses are appended. In the fifth chapter the emotional response of the experimental animal to its own interpretation of conditioning signals is discussed, and stages in the onset of emotional breakdown are identified. In his final chapter, Liddell elaborates his observations of the behaviour of front-line soldiers under battle conditions in the light of the behaviour of his experimentally disturbed animals, and adds some comments on the role of pleasurable emotions in mental health.

Much of the material gathered together in these lectures is familiar from Liddell's previous writings. The style is conversational, and, while experimental findings are frequently cited, they serve to illustrate rather than to convince. Anecdotal evidence is not eschewed. There is no index, but a selected bibliography of nineteen references is provided.

The author's viewpoint has changed over the years, and this change towards his present emphasis on the importance of the conditioning situation as such in the aetiology of emotional disturbance is epitomized in his statement that 'the conditioning routine which we regarded as innocuous apparently meant murder to the sheep'. But his criteria of what constitute an 'experimental neurosis' are loosely defined, and, indeed, appear in this book to fluctuate.

It is difficult, therefore, to see to what audience the book is addressed; experimental psychologists will find it insufficiently organized to serve as a useful treatment of Liddell's undoubtedly major contribution to the experimental literature in this field. This reviewer's bias would lead him to attribute this to Liddell's failure to provide or employ a theoretical system which might illuminate his empirical data and be enhanced in turn thereby. On the other hand, the book does not appear to offer psychiatrists much insight into the part played by emotional strain in the development of neurotic disorders, beyond some simple animal analogues. Some, however, may find these satisfying and helpful.

P. L. BROADHURST

The Art of Making Sense. By LIONEL RUBY. London: Davies. 1956. Pp. 285. 16s.

This is one more of those books which aim at improving the reader's mind by the administration of formal logic in graduated doses, heavily diluted with anecdote. Of its kind it is reputably done. Unlike some authors in this field, Prof. Ruby avoids the pretence of identifying logic with the psychology of thinking,

does he attempt to peddle the subject as a royal road to truth. On certain points he is undeniably superficial, notably in his off-hand treatment of verbal meaning and casually uncritical acceptance of the correspondence theory of truth, but on the whole he covers the ground fairly, within the limits that a reader can be expected to understand.

The opening chapters are mainly concerned to pin down argumentative misdemeanours and non-verbal fallacies of semantic or psychological origin. This is followed by a straightforward account of formal reasoning, syllogistic and hypothetical, supplemented by a fleeting glimpse of symbolic methods. A rather curious defence of the law of excluded middle against the clamours of Korzybski and his followers—unnecessary at this level, one would have thought, and certainly out of place in an English edition, for the non-Aristotelian Count is by no means the celebrity here that he is (or was) in the States. The remainder of the book is devoted to probability, evidence, induction and scientific method, with a chapter on value judgements; the sections on gambling, legal inference and 'the logic of Sherlock Holmes' (a remarkably ill-informed theorist, by the way) are all interestingly put over, though English readers might well have been given a footnote on the rules of 'craps' and readers of all kinds may wonder, with the whole history of science at his disposal, the author has chosen to pick his examples of 'what the scientist thinks' from Dr Cronin's *Citadel*.

The first duty of a popular logician is to keep his audience awake. Prof. Ruby knows this, and sparkles conscientiously at about three corny stories to the page; all the same, one cannot help feeling that the book would be even more readable if it were less obviously anxious not to be dull.

P. L. HEATH

Nature of Sympathy. By MAX SCHELER. Translated by Peter Heath. London: Routledge and Kegan Paul. 1954. Pp. liv+274. 30s.

In addition to the series 'Rare Masterpieces of Philosophy and Science' is of less direct interest to philosophers than its title would suggest. On the other hand, when time and inclination offer it is rarely profitable to explore somewhat alien rumination. Whether Scheler's book is a masterpiece or even the ordinary writing of a master seems to the present reviewer open to question. It has, however, historical interest and in the light of Gardner Murphy's recent speculations on the limits of personality it even has a topical interest—its very confusions reveal the pitfalls awaiting the unwary. Quite unquestionable is the value of Dr Stark's excellent introduction.

At the time of writing 'Sympathy', Scheler argued that there are various levels of human understanding and up to what he calls the 'vital' level there is no real problem about our sympathy with or knowledge of other people because direct awareness is primary. This looks interesting until one realizes the scholastic context. In 1913, still retaining the Thomism he later abandoned, Scheler went on to maintain that there are spiritual as well as vital levels of existence and understanding. And at the spiritual level there is an essence or core of every personality which, if it is ever known, will only be apprehended through a very subtle form of 'fellow feeling'. This for Scheler presupposes an equally subtle notion of knowing and recognizing another person as forever a distinct and independent entity.

Scheler tries introspectively to distinguish other varieties of relation apt to be left unscrutinized in English because English, unlike German, has no ready-made words like *mitleid* or *mitfreude*. Difficult though he is, Scheler can be persuasive and his discussion is occasionally illuminated by flashes of insight which give one pause. But one should not be blinded by artistry. And torn from the context of what in German seems deeply confused philosophizing Scheler's views on direct awareness, for example, hardly stand up to psychological scrutiny (beyond his sensible attempt to defend some element of social responsiveness as primary). Moreover, in the cold clear light of an English morning the idea that other people have love, fellow feeling or respect as independent entities appears either vague or suspiciously invidious.

In sum, this reviewer has doubts. But it is fair to say that the doubts are partly inspired by the value of other volumes in the same series under Dr Stark's general editorship. Inevitably one compares. And one would have Scheler when they can have Aristotle and Aquinas commenting or, for that matter, Machiavelli of the *Discourses*?

J. WYNN REEVES

Migration and Mental Disease. By BENJAMIN MALZBERG and EVERETT S. LEE. New York: Social Science Research Council. 1956. Pp. x+142. \$1.50.

The authors of this book study the relationship between migration and mental disease by means of statistical analysis of records of first admissions to hospitals for mental disease in New York State during the three years 1939-41. In all, 46,000 cases are included, classified into three main groups: dementia praecox, manic-depressive psychoses and all other psychoses. Migrants are variously defined (a) foreign-born, (b) having been born in another state, (c) having migrated within the five-year period 1935-40. Rates of first admissions in the area of destination for migrants thus defined are compared with rates of first admissions for the 'indigenous' population of New York State. Age-standardized rates are used in comparisons. The authors are careful to point out many limitations in their data, and the danger of drawing unwarranted conclusions.

In general, migrants are found to show higher rates of first admission than non-migrants, whichever definition of migration is used. It is inferred that this differential holds for cerebral arteriosclerosis and senile psychoses as well as for the 'functional' psychoses. For foreign-born men the rate is relatively high for dementia praecox but not for other psychoses, while for foreign-born women the rates for all groups of psychoses are relatively high. For inter-state male migrants, the rate for dementia praecox is again high, while for inter-state female migrants the rate for manic-depressive psychoses is also high. Women show larger overall differentials than men. With the exception of dementia praecox, the rates for native (inter-state) migrants are higher than for foreign-born. Rates are much higher for recent arrivals.

Although the data do not enable one to decide between the two most plausible explanations of the differential rates—that they are due to initial selection, or to the problems of adjustment after migration—one finding appears to have some bearing on the hypothesis that schizophrenics tend to migrate more readily. The rate of admission for dementia praecox is low relative to rates for manic-depressive and other psychoses during the first year after migration.

The book has an introduction of some forty pages by Dorothy Thomas, which provides a compact and valuable account of previous work on the problem.

D. GRAHAM

Crime and Social Action. By GEORGE GODWIN. London: Watts and Co. 1956. Pp. vi+271. 18s.

This book sets out to gather together various contributions to criminology made over two centuries from widely diverse fields.

Part I examines the nature of the criminal on the basis of a somewhat arbitrary selection of investigations ranging from Lombroso to Wertham as the representative of the modern psychiatric approach. This section ends with an emphasis on heredity as the paramount factor in producing the 'criminal diathesis', the criminal being merely the inferior man. Part II is an account of the development of penal methods from Beccaria, through such English reformers as Fry and Howard, to a somewhat unfair assessment of the work of East and Hubert as a contribution to the development of current methods of psychological treatment for offenders. Part III, on 'Murder and Madness', consists essentially of a discussion on how far the M'Naghten rules are out of date as a basis for dealing with problems of insanity and criminal responsibility. To this is appended a somewhat tendentious chapter on the death penalty with special reference to such topics as the Evans case of recent years. Part IV is a light-weight account of detection and the forensic 'sciences'.

It is too much to accept the claim that all this constitutes 'a very comprehensive and well-documented study of social attitudes to the criminal' with the author's method transforming 'what might otherwise have been a book for the specialist into one with a broad appeal to the general reader'. Mr Godwin's technique is a mixture of popular journalistic reporting, dramatic historical reconstruction, *Readers' Digest* condensation of information and ideas and quotations from original sources. The general presentation of all this material is muddled and poorly integrated, with a number of inconsistencies and loose ends which remain after his final humanitarian-scientific summing-up. A preference for writing in short, chopped paragraphs tends to add to the book's general lack of cohesiveness.

Though specialists will find most of the book flimsy, its general emotional impact on the non-technical general reader will probably be to leave him with a vaguely more 'enlightened' understanding of problems of crime and criminals.

R. L. MORRIS